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SURGICAL SHOCK.[1]

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May I first express my appreciation of the honour I have received in being asked to deliver this paper to the members of the Branch?

I experienced some difficulty in choosing a suitable subject, as my personal experience and notes for the last few years have been chiefly derived from war surgery and others have already addressed you on various aspects of their surgical experience at the war. But, as far as I know, surgical shock has not been dealt with by any of these and it is a subject of equal importance and interest in civil as in war work.

"Surgical shock," or, as Cannon has suggested, "wound shock," is one of the subjects on which much work and research was conducted during the war. The large numbers of wounded dealt with presented opportunities for accurate and systematic investigation on a scale only possible in war. The observations and records obtained have not only greatly advanced our knowledge, but have also provided material to be still further drawn on by the numerous workers on this subject. "Team" work by surgeons, physiologists and pathologists proved of very great value.

It may be of interest to relate the organization which was created for the special investigation of this subject. In the spring and summer of 1918 I was in charge of a surgical team in the British Third Army, near Amiens; in that army the consulting surgeon, Sir Henry Gray, by his energy and enthusiasm, inspired and encouraged the surgeons under him to investigate cases of shock critically and systematically. Attached to our group of casualty clearing stations was a "shock centre," in charge of Captain Kenneth Walker, R.A.M.C., who had under him a small staff and all the necessary equipment for the investigation of these cases and for the preparation of the various solutions used in their treatment. Captain Walker was also Secretary of the Shock Committee, on which were representative medical officers of the Army; this Committee was in constant and close touch with the Medical Research Committee in London, which was investigating the same subject experimentally. Professor Bayliss and Dr. Dale were directing the work in London and the results of their experimental work in the laboratories and the clinical observations in the Army were kept in close touch with each other. The results of the work were published from time to time and much of what I have to say to-night is drawn from their work, as well as my own personal experiences in applying this work in the treatment of cases of wound shock.

Perhaps I should say at the outset that, even now, there is no comprehensive theory which satisfactorily accounts for the recorded and observed phenomena of shock. Investigations prior to and during the war have conclusively demonstrated that none of the numerous theories in the field are any longer tenable, but a very great advance has been made in the treatment of cases of shock. The results obtained towards the end of the war were greatly in advance of those of the earlier years; and also the solution of the problem of the causation of shock has been appreciably assisted.

[1] Read at a meeting of the Victorian Branch of the British Medical Association on March 3, 1920.

Definition.

The definition of shock is that depressed activity of the bodily functions, which frequently follows upon severe injury, either by wounds or in surgical operations, and also after hæmorrhage from any cause (Bayliss).

Note the inclusion of hæmorrhage in this definition. Shock and hæmorrhage in war wounds are almost always associated in varying proportion, but apart from obvious hæmorrhage, I hope to be able to show that there is the same underlying condition in both. The end result of each is defective blood supply and especially defective oxygen supply to the vital organs of the body.

One further point must be made clear. Primary shock and secondary shock are to be differentiated. In many cases this may be very difficult. My remarks to-night are on the question of secondary shock. In 1870 Goltz demonstrated his classical experiment of "primary shock," in which he showed that a blow on the exposed mesentery of a suspended frog caused arrest of the heart through the vagus and loss of arterial tone, so that blood tended to accumulate by gravity in the splanchnic area. This is a phenomenon of reflex inhibition, analogous to fainting. As is well known, it may also set in rapidly after severe injury, pain or emotion. But after recovery from primary shock, secondary shock may develope, or primary shock may pass gradually into secondary shock, or secondary shock may gradually develope without the manifestation of primary shock.

To put the problem concisely, in shock the following apparent paradox exists: a falling blood pressure, with contracted arteries, a normal heart and a normal vasomotor centre. Most important of all, there is a deficiency in the volume of blood in circulation, which not only applies to cases of actual hæmorrhage, but also to those where there is no reason to suppose there has been any great loss of blood.

It has been shown that none of the following theories are compatible with the facts. I do not propose to discuss these in detail, but they are of interest historically, as well as showing the different aspects from which the problem has been approached. In some of them secondary results of shock have been regarded as primary causes, while in others the supposed facts on which the theories are based, have been definitely disproved.

Theories of Shock.

I.—Exhaustion of Bulbar Centres and Especially the Vasomotor Centre (Crile and Lockhart Mummery).

This is disproved because:—

(*i.*) The bulbar centres can be shown to be of nearly normal excitability for some time after shock has developed.

(*ii.*) The peripheral arteries, as first shown by Malcolm, are contracted and not dilated.

(*iii.*) Although specially searched for, there is no evidence at abdominal operations of the dilatation of the splanchnic vessels which was supposed to exist.

It cannot, however, be denied that, comparable with Goltz's experiment, there may be a reflex peripheral vasodilation in the initial stage of shock.

II.—Heart Failure.

All observers are agreed this is not the cause.

III.—Acapnia Theory of Henderson.

That, owing to the violent respiration caused by pain, the carbon dioxide in the blood is so reduced (and therefore the hydrogen ion concentration of the blood) that the stimulus to the respiratory centre is deficient. This is an ingenious theory, but (*i.*) there is nothing like the excessive respiration necessary to cause such a diminution of carbon dioxide and (*ii.*) shock does occur with a Clover or similar inhaler where excess of carbon dioxide is breathed.

IV.—Acidosis Theory.

As will be shown later by Bayliss's work, the injection of lactic acid (the acid usually incriminated) or other acids is innocuous. In fact, "acidosis" is a purely secondary result of diminished oxygen supply to the tissues and is not, as certain investigators have claimed, the primary cause of shock.

V.—Suprarenal Exhaustion.

As a matter of fact, there is an increase and not a decrease of circulating adrenalin in experimental shock and this is what one would expect in wounded men keyed up to the high pitch of excitement in battle.

In actual practice the lowered blood pressure is the most important feature of shock, and in both experimental and clinical work, if the blood pressure is restored and maintained, the other symptoms disappear. In order that the body tissues may carry out their functions it is vital that there should be:

(1) An adequate supply (quantity) of circulating blood.

(2 A sufficient blood pressure.

By these means the cells of the body are supplied with the most vital constituent of the blood, *viz.*, oxygen, and if they are deprived of an adequate oxygen supply for a period, which is probably not more than an hour or two, harmful metabolic changes occur and irreparable damage results. Bayliss has shown that 1½ to 2 hours' exposure to a blood pressure from 70 mm. downwards in the cat resulted in paralysis of the bulbar centres, at first respiratory and then vasomotor. In another experiment in which the blood pressure was 58 mm. for one hour, the respiratory centre failed to recover on transfusion. In man it seems likely that the vasomotor centre is affected before the respiratory centre.

In addition to the lowered blood pressure, it has also been shown that there is a diminished volume of blood circulating in the vascular system. This can be observed by the "vital red" method and is true, not only for cases in which there has been actual hæmorrhage from wounds, but also in those where none has occurred. For this reason Cannon has suggested the name "exæmia" for the general condition.

It is also found that, in addition to the blood volume being reduced, there is an increased hæmoglobin and red corpuscle content of the blood in the capillaries, as compared with that in the veins. Instead of the normal difference of 3% there is often a difference of 30%, clearly showing that there is a concentration of the blood in shock, as well as a diminished volume, due to plasma escaping from the vessels and perhaps also by sweating. This percentage increase of red corpuscles in shock had been observed experimentally by Sherrington many years ago; it has been confirmed by Malcolm, who also maintains that the arteries are constricted rather than dilated in shock. This has been confirmed by the experimental work of American observers—Seelig, Lyon and Yandell Henderson—and also clinically by surgeons and pathologists in France during the war.

Further, the apparent defect of blood volume was serious, out of all proportion to the loss of plasma which the concentration indicated. There are certain injurious factors which tend to lower the blood pressure, but are not necessarily serious, unless combined with a comparatively small loss of blood and *vice versâ* an unimportant loss of blood may become serious when combined with one or more of these other factors. Bayliss investigated this aspect of the case experimentally and found that in cats the removal of one-fourth of the blood is rarely followed by any dangerous consequences, but when this is combined with other factors themselves also attended by some degree of fall of blood pressure, such as cold, injury, etc., a much smaller loss of blood than one-fourth results in a permanent and serious fall of blood pressure, attended by other signs of wound shock.

Contributing Causes.

Of the contributing causes, the following are of great importance and call for special attention in treatment:

I.—Exposure to Cold.

It is agreed by all clinical observers that this is one of the most important factors in exaggerating wound shock.

Experimentally Cannon and Bayliss found that cats could be cooled to 25° C. rectal temperature and completely recover by warming, but this was not the case where cold was combined with a loss of 23% to 30% of blood. The effect of cold is to lower the blood pressure, depress the vital functions and produce a slowing of the heart beat.

II.—Injury to the Tissues and Especially Muscular Tissues.

From Bayliss's experiments this form of shock produced experimentally was most closely similar to that met with in wounded men, especially when combined with slight hæmorrhage. Surgeon-General Cuthbert Wallace and other surgeons had noted that operations or wounds involving injury of large masses of muscular tissue, were especially liable to produce shock. It is well known that in amputations of the lower

limb shock increases as the body is approached, while, on the other hand, an interscapulo-thoracic amputation is not accompanied by much shock. The actual size of the wound in the latter is not far removed from that of amputation at the hip joint, but the difference consists in the amount of muscle cut through. The especial liability of shock in multiple wounds involving muscle and compound fractures of the thigh has also been noted. Bayliss and Cannon investigated this experimentally by crushing the thigh muscles of cats and found that a progressive fall of blood pressure and other signs of shock resulted. Section of the spinal cord above the origin of the limb nerves did not prevent the result, whereas clamping the main artery and vein did so. They therefore concluded that some chemical product of tissue injury must be absorbed into the circulation from the injured tissues. McNee had also observed that excision of injured parts, or even the prevention by a tourniquet of blood from entering the general circulation, was followed by marked improvement.

During the infliction of the injury the blood pressure falls and if the fall is not very great, spontaneous recovery may occur; but in more severe cases the primary fall is usually followed by a slow secondary fall, ending in death. In either case a slight hæmorrhage (22% in one experiment) has the effect of enormously exaggerating the state of shock.

This conception of some substance or substances circulating in the blood and producing the picture of shock was not a new one. Dale and Laidlaw had been working since 1910 on a base, histamine, which has the remarkable effect of powerfully dilating the capillaries, but not the arterioles, and in larger doses produces a condition of profound shock. There is reason for believing that substances of this nature are produced in injured and disintegrating tissues. The action of histamine is also similar to that of a large class of protein cleavage products formed by partial digestion or bacterial action. Products of this nature have been extracted from almost all organs of the body. They are naturally present in great abundance in the contents of the small intestine; they appear to be set free readily from almost any tissue as the result of injury or even of temporary stoppage of the circulation.

III.—Anæsthetics.

Surgical operations exaggerate the state of shock. It is of the greatest importance that cyanosis should not be permitted to occur, as the body tissues are then exposed to all the evil effects of diminished oxygen supply. It is well known, as pointed out by Mathison, that the initial effect of excess of carbon dioxide is to cause a rise of blood pressure by stimulation of the vaso-constrictor centre, but, if continued, it was shown by Patterson that a pronounced fall of pressure follows, owing to its injurious effect on the heart, with peripheral vaso-dilatation.

On the other hand, defect of oxygen, even when there is no excess of carbon dioxide, is very apt to leave behind a condition in which there is a progressive fall of blood pressure.

Haldane is inclined to think that administration of oxygen might be of value in shock. It is not so much that the arterial blood is insufficiently oxygenated, but that the blood is not supplied to the tissues with sufficient rapidity, owing to the low blood pressure; the more effective treatment is to raise the blood volume by increasing the volume in circulation.

Yandell Henderson's suggestion of increasing the carbon dioxide in the air breathed might reasonably be expected to be of use where the respiratory centre excitability is diminished; the excess present in the blood would not materially prejudice the amount of oxygen taken up at the alveolar tension. The rebreathing of expired air, with its defective oxygen, is without a doubt deleterious and not to be recommended under any consideration.

In the time at my disposal I do not propose to consider in detail other contributory causes of shock. These will, however, be sufficiently evident from the remarks on treatment.

Treatment.

We have seen that the blood pressure is the chief clinical guide in the treatment of shock and there are two ways in which this may be raised:

(1) We may use drugs which constrict arterioles, the peripheral resistance being thus increased; the same force of heart beat as before produces a higher pressure.

(2) We may raise the pressure by increasing the volume of circulating blood, without altering the peripheral resistance.

We have also seen that the object is not to raise the blood pressure in itself, but to insure a better blood supply to the tissues.

With regard to the use of vaso-constrictor drugs, such as ergot, adrenalin, pituitrin, etc., it is evident that any increase of blood pressure resulting from their use is more or less counteracted in any particular organ by the constriction of the arterioles of that organ. Further, as has already been stated, the evidence is strong that the arteries are already contracted and surgical observation has failed to find evidence of accumulation of blood in the splanchnic area in cases of shock. No good object would, therefore, be served by causing these vessels to contract.

But, by the other method, *viz.*, by increasing the volume of circulating blood, a better supply to the organs and especially to the cerebral centres is insured. By intravenous injection of fluid in sufficient quantity the blood pressure is raised, the fluid lost to the body by obvious hæmorrhage is restored and also that lost to currency by stagnation in the capillary areas.

Until recent years, artificial fluids, such as saline solution of varying strengths, Ringer's solution, etc., have been used for this purpose, but it has been conclusively shown that such solutions are useless for restoring blood pressure in cases of shock and, further, are often harmful. I have frequently observed that a temporary mechanical rise of blood pressure, produced by introducing a litre or more of saline solution into the circulation, is rapidly followed by a fall which may, and generally does, proceed to a lower level than before the injection. This is confirmed experimentally by the work of the Medical Research Committee on cats. Similar results are obtained when Ringer's solution is used instead of saline solution,

In fact, Sherrington and Copeland showed many years ago by measurements of the specific gravity of the blood that salt solutions leave the circulation in less than half an hour. The reason why so-called "isotonic" solutions, such as saline or Ringer's solution, leave the circulation rapidly is because they do not replace the colloids (chiefly proteins) which have been lost from the blood. These colloids have an osmotic pressure of between 30 and 40 mm. of Hg.. By reason of this, if blood is separated from water or Ringer's solution by a membrane impermeable to colloids, but permeable to salts, water will be attracted with a certain force. The walls of the blood vessels are composed of such a membrane and as long as the blood pressure in the small arterioles is greater than the osmotic pressure of the blood colloids, the tendency to attract water is overpowered and lymph passes out of the vessels by filtration. Lymph has the composition of blood, minus colloids and corpuscles. But, as we follow the blood in its course towards the capillaries, we reach a region where the filtration pressure of the blood is equal to the osmotic pressure of the colloids and still further on a region where this blood pressure is lower than the osmotic pressure and lymph is reabsorbed in part and the remainder passes by lymphatic channels, to be carried back to the blood *viâ* the thoracic duct. Therefore, the net result of diluting blood with a solution containing no colloids is that filtration into the tissues is accelerated, the area where filtration occurs is increased and that of reabsorption reduced. Disappearance of liquid takes place from the blood, with its accumulation in the tissue spaces. This disappearance of injected liquid from the blood can be easily detected by a series of hæmoglobin estimations at intervals after the injection. These show that the hæmoglobin percentage quickly begins to rise again after the introduction of the solution.

Other observers have recommended the use of hypertonic solutions, such as were used by Leonard Rogers in the treatment of cholera, in which large quantities of water are lost from the intestine and in which the specific gravity of the blood is much increased. Whether the blood may become so concentrated by loss of liquid in cases of severe wound shock at a late stage is a disputed question, but there is no evidence that it occurs at the time when treatment is usually applied. In experimental work the temporary rise of blood pressure produced by the introduction of hypertonic solutions lasts very little longer than that of Ringer's solution, as it is soon counteracted by water attracted from the tissues and, of course, the diminished colloid concentration acts as before.

On the assumption that "acidosis" is a factor in the causation of shock, others have substituted alkaline solutions, such as sodium bicarbonate (3% to 4%) for saline solution. It is undoubtedly true that, owing to defective blood supply and consequent insufficient oxidation in the tissues, acid products are formed, which diffuse into the blood and are there neutralized, thereby reducing the bicarbonate in the blood and what has been called the "alkali reserve." An acidosis as great as occurs in shock may occur after severe muscular exercise and is also met with at high altitudes, but under normal conditions is rapidly removed. The low blood pressure and deficient oxygen supply to the tissues in shock is the cause of the "acidosis" and it is more rational to increase the oxygen supply by improved circulation than by attempting to neutralize the acid once produced. The "acidosis" in itself is innocuous and may even be beneficial in that the increased hydrogen ion concentration of the blood may stimulate the respiratory centre to increased activity and so increase the supply of oxygen.

One other factor should be mentioned. This is the question of the viscosity of the circulating fluid. The peripheral resistance in the blood vessels is due to the internal friction of the contained fluid and a decrease of viscosity, such as is produced by diluting blood with limpid fluids, results in a fall of peripheral resistance and consequent lowered arterial pressure. Ringer's solution and water have a viscosity which is only one-third that of blood.

We therefore see that solutions which only contain crystalloids have two defects:—

(1) They contain no colloid with an osmotic pressure.
(2) Their viscosity is too low.

Bayliss experimented with various solutions which should not possess these defects.

Glycerine has sufficient viscosity, but is diffusible. It also has a deleterious effect on the heart and corpuscles.

Starch and agar are indiffusible, but have such large molecules that their osmotic pressure is practically nil.

Foreign proteins have the objections that they affect the kidney and are excreted in the urine. There is also the question of anaphylaxis, especially when one remembers that all wounded men have had antitetanic serum injections.

Gelatine (6%) and gum (7%) have the same osmotic pressure and also the same viscosity as blood. The osmotic pressure of these colloids is not in themselves sufficiently high to prevent hæmolysis of red corpuscles, so they must be dissolved in 0.9% sodium chloride. Gelatine has the objection that on sterilization it loses much of its viscosity and there are also the possibility of unkilled tetanus spores and the well-known risk of intravascular clotting.

For the above reasons gum arabic was decided on. It is innocuous, non-hæmolytic and non-agglutinating and easily sterilized, without loss of viscosity.

The preparation of gum solution is as follows: The gum is dissolved over a water bath. If freshly distilled water is not available, tap water may be used without harm. The solution should be filtered through flannel or several layers of gauze. In earlier work 3% solution was used, but better results are obtained by using a 6% to 7% solution, which is, as has already been stated, of the same osmotic pressure and viscosity as blood. The technique of introduction is the same as for intravenous injection of saline solution. The solution is not allowed to run in at too rapid a rate; 600 c.cm. in a quarter of an hour is a safe standard. As the amount of blood lost is uncertain, it is best to control the effect of injection by watching the blood pressure. Half a litre may be

given as a routine procedure and a similar quantity one-half to one hour later.

While Bayliss's gum solution is of great value, yet after all it is merely an inert solution, which restores the volume of the circulating fluid. Blood transfusion, in which actual blood is restored to the circulation, is much more effective and to be preferred wherever possible. Canadian and American surgeons were first responsible for the rapid and wide use of this most valuable method of treatment. There is no doubt that blood transfusion must have an important place in civil practice. The chief difficulty to its general application is that of obtaining suitable donors at the moment they are needed.

Blood transfusion is really the simplest example of transference of living material from one person, called the donor, to another person, called the recipient. It is comparable to the different forms of grafting, in which skin, bone, etc., are transferred from one individual to another and live in their new host. We have every reason for believing that the blood corpuscles administered in blood transfusion live and perform their usual functions in the circulation of the recipient. Blood films taken at intervals after transfusion show no evidence of abnormal corpuscles. There is no evidence of free hæmoglobin in the circulating blood and no hæmoglobinuria. Animals may be repeatedly bled and transfused with blood from another animal to an extent which can only be possible on the assumption that the transfused corpuscles live and functionate. The transfused blood not only replaces blood volume, it also supplies the active functioning tissue which has been lost. As previously stated a prolonged low blood pressure results in a loss of excitability of the bulbar centres and, if this has lasted more than a certain time, no recovery is possible, even by blood transfusion. It is evident therefore that to be effective, it must also be prompt.

It is well known that animals' blood cannot be used for this purpose, as severe toxic symptoms are produced. Further, the blood of any donor cannot be used until it has been proved to be compatible with that of the proposed recipient. Moss showed that early in life the blood of each individual takes up the character of one or other of four groups and these probably remain fixed throughout life. The groups are numbered 1 to 4. The blood of a donor belonging to any one group may be safely given to another person of the same group, but not necessarily to a person of another group. The incompatibility manifests itself in hæmolysis and agglutination and these two reactions run parallel with each other, so that it is only necessary to test for the one or the other. In actual practice the agglutination reaction is used. The blood grouping may be shown in the form of the following table:—

Red Corpuscles.

Serum.	1	2	3	4	Percentage.
1	—	—	—	—	8
2	+	—	+	—	40
3	+	+	—	—	10
4	+	+	+	—	42

+ = Agglutination. — = No agglutination.

It is found that the important thing in practice is the possible agglutinative action of the recipient's serum on the donor's corpuscles. The donor's serum is so greatly diluted by the serum of the recipient that any possible agglutinating effect on this score is of no practical account.

From the above table it is seen that the corpuscles of Group 4 are not only compatible with their own group, but also with the sera of each of the other three groups. For this reason they may be called "universal donors' 'and fortunately they represent nearly one-half of all individuals.

The simplest method of testing a prospective donor is as follows: Two drops of previously tested "2" and "3" sera are placed on a glass slide, one towards each end of the slide. A drop of blood from the donor is mixed with each drop. Within two minutes agglutination or non-agglutination of his corpuscles is quite evident to the naked eye.

There are two possible methods of transferring blood from one individual to another—direct and indirect. In all methods of blood transfusion the difficulty to be overcome is clotting.

In the direct method an artery of the donor is connected to the vein of the recipient, either directly or by means of fine rubber tubing, with a cannula at each end, as in the method of Fullerton and Bazett. The great objection to this method is that it is not possible to tell how much (if any) blood passes. There is the added objection that donor and recipient must be side by side and clotting is more liable to occur. The indirect methods have entirely superseded the direct.

In the indirect methods the blood of the donor is received in a vessel or syringe in which steps have been taken to prevent cloting. A quantity which can be accurately measured, is transferred to the recipient.

The two most commonly used are the paraffin and the citrate methods. Both are equally effective. While at the Base I used the paraffin method and when at the casualty clearing station I used the citrate method, which is, I think, more applicable to civil work, being simpler in technique and more certain.

Paraffin Method.—The blood of a previously tested donor is received into a glass flask which has been coated inside with an even layer of paraffin of a melting-point between 40° and 50° C.. Great care must be taken in the cleaning and paraffining of the flasks. A tube of the Kimpton Brown type, with a capacity of 400 to 500 c.cm. is the most useful, although when these were not available the large ampoules used as containers in the Carrell-Dakin treatment were readily improvized and proved very satisfactory. The vein (usually median basilic or cephalic) of the donor and recipient are exposed by open dissection under local anæsthesia. The vein is isolated with a minimum of dissection, cleared and ligatured. A V-shaped incision is made in the vein of the donor, distal to the point of ligature and the drawn out end of the glass flask inserted. The flow of blood into the flask may be assisted by producing a small negative pressure by means of a rubber bulb. When the required amount of blood (500 to 1,000 c.cm. usually) has been withdrawn, the flask is removed, the vein of the donor tied distal to the opening and the blood is transfused into another V-shaped opening made in the vein of the recipient proximal to the ligature previously ap-

plied. The flow of blood into the vein of the recipient may be hastened by connecting the other end of the rubber bulb before mentioned, by which a slight positive pressure is developed in the flask.

Citrate Method.—In this method (Robertson's) is required a large bottle with a well-fitting rubber cork, through which three tubes are led: (1) Conducting the blood from the donor to the bottle. (2) Conducting blood from the bottle to the recipient. (3) Connecting with a rubber bulb already described under the paraffin method and used for producing negative or positive pressure in the bottle, as is required. It is essential that all parts of the apparatus be kept thoroughly cleaned and all cannulæ and tubing stored until required in sterile paraffin. Firstly 160 c.cm. of sterilized 3.8% sodium citrate solution are placed in the bottle; into this the blood from the donor is received. While the blood is flowing the bottle is kept gently agitated. It is also kept warm by being immersed in a containing vessel holding water at a temperature of about 100° C..

The citrate method has the following advantages over the paraffin method: (1) The donor and recipient need not be together, side by side in the operating room, as is the case in the paraffin method. (2) The blood in the citrate method may be kept and safely used for some hours after it is withdrawn. I have used such blood as long as six hours after it was taken from the donor. No hæmolysis of corpuscles occurred. Fears have been expressed that the amount of citrate thrown into the circulation may have a harmful effect, but no harmful effects have been observed.

The citrate method is so generally applicable that its use was greatly extended and citrated blood given in the advanced dressing stations of the field ambulances and even at the regimental aid posts. Very valuable work was done on these lines by the "resuscitation teams" which were organized in each of the Australian Divisions.

Time does not permit me to go into further details of the practical points of blood transfusion, but mention must be made of the importance of gas and oxygen anæsthesia in operating in desperate cases of shock. Blood transfusion combined with gas and oxygen anæsthesia rendered successful operation possible in cases absolutely impossible under any other method of anæsthesia.

In conclusion, as Bayliss puts the matter, we may summarize the present state of our knowledge of wound shock as follows: Various causes in combination, some nervous, some chemical, each associated with a reduction of arterial pressure and all exaggerated by hæmorrhage, result in a state of collapse, whose symptoms seem to be sufficiently accounted for by the effects of a more or less prolonged low blood pressure. Along with hæmorrhage, the most serious of these collateral causes is the absorption of toxic products from injured tissues, especially muscle. These products have a dilator effect on the capillaries, similar to histamine; blood is withdrawn from circulation and held up in the capillaries by stasis. The condition becomes progressively worse, unless the continued inflow of toxic products is prevented or counteracted. The injured parts should therefore be removed as soon as possible, operative treatment and resuscitation being undertaken at the earliest opportunity. The toxic products already absorbed may be eliminated or destroyed if the blood pressure and volume of blood in circulation be raised by appropriate intravenous injection. If, however, the low pressure has lasted for some time, the nerve centres become paralysed and structural changes are evident.

A PLEA FOR A STANDARD OF CURE IN CASES OF GONORRHŒAL URETHRITIS IN THE MALE.[1]

By **V. N. B. Willis, M.B., Ch.M. (Sydney),**
Brisbane.

Owing to the recent demobilization of large numbers of men from the army, many of whom during their period of service suffered from gonorrhœa, the general practitioner is likely to be consulted as to whether they are free from infection and fit to marry.

Many articles have been published lately on the subject of gonorrhœal urethritis, introducing certain new methods of treatment, for instance, the use of detoxicated vaccines, irrigation treatment with solutions of acriflavine, the intramuscular injections of colloidal manganese, etc.. Each author draws deductions, in some cases from large series of cases, as to the efficiency of the different methods of treatment, but as no one definite standard of cure has been obtained by all the writers, it is extremely difficult to compare their results.

The following article aims at a standard which would eliminate, as far as is at present possible with the means at our disposal, any hidden foci for the gonococcus in the genito-urinary tract. It has been written with two objects in view. In the first place to impress upon the members of the medical profession the absolute necessity for a thorough and complete examination of all men suffering from urethritis, before telling them that they are non-infectious (see Appendix); and in the second place with the hope that if some such standard of cure were adopted by those who advocate newer methods of treatment, it would be possible to draw accurate conclusions from their work and thus lead to the adoption of procedures which would bring about a rapid cure of this disease. It is not claimed to be original in any way, but as it has been used by the writer for some time past in a military hospital, a description of it may be of interest.

On looking up the literature of gonorrhœa I find that different authorities on this subject have different standards of cure.

1. Harrison, in "Venereal Disease in General Practice" (1918):—

(*a*) There must be no gonococci in urethral smears or in the centrifuged urine after the patient has been drinking beer for a few days.

(*b*) There must be no growth of gonococci from the centrifugalized deposit from the urine of the patient.

(*c*) There must be no gonococci after an irritant injection of solution such as 5% protargol in the urethra.

(*d*) There must be an absence of general reaction in the form of a rise in temperature and of focal reaction in the form of an increase in the urethral discharge in which gono-

[1] Read at a Meeting of the Queensland Branch of the British Medical Association on March 5, 1920.

cocci may be found after a provocative vaccine injection of 150 to 200 million.

(e) The patient must give a negative deviation of complement reaction.

(f) After these tests have been repeated, the patient may with greater confidence be given a clean bill of health.

2. Watson, "Gonorrhœa and its Complications" (1914):—

(a) Negative slides and cultures of any urethral discharge after the patient has held his water all night, if possible.

(b) If the urine contains shreds examination with the urethroscope is necessary to locate the lesions.

(c) The secretion obtained by massage of the prostate and vesicles should be normal, macroscopically and microscopically.

(d) There should be no nodules in the epididymes.

Should the complete examination reveal no evidence of disease and no subjective symptoms be complained of, permission to marry is warranted, but in the case of any doubt, one or more of the methods of provoking gonococcal activity may be adopted and the skin and complement fixation test employed.

3. Koll, "Disease of the Male Urethra" (1918):—

(a) The patient should give negative slides and cultures from his urethral discharge after instrumentation, alcohol tests and strenuous physical exercise and also from his urine passed after massage of the prostate. If the infection is limited to the anterior urethra, one year must have elapsed from the time of the last evidence of the infection before consent is given for marriage. If the infection extends into the posterior urethra, then two years must elapse after the final clinical disappearance of the disease.

(b) He also states that some authorities still believe that it can never be definitely known when the gonococcus has disappeared.

(c) He quotes E. L. Keyes who claims that "he has never known the gonococci to persist in the male urethra for more than three years, while in at least 90% of cases it disappears, with or without treatment, in one year."

4. Asch, "Gonorrhœa," states: "It is my rule never to discharge as cured a patient before having given him a provocative vaccine injection followed by urethroscopic examination in the course of one or two days."

5. Barringer, in "Modern Urology" (Cabot), under evidence of cure, states:—

(a) Practically all urologists believe that a pus-free urine means a urethra free from gonococci, but most urologists insist that a patient remains pus free a certain length of time before he can be considered cured; and adds that Keyes puts this period at three months.

(b) If three months have not elapsed, or if the patient has free pus or pus shreds in his urine, or pus in the secretion expressed from his prostate and seminal vesicles, then a negative culture or complement fixation test is necessary to declare the patient gonococcus free.

6. Luys, "Text Book of Gonorrhœa," in his preface states that urethroscopy is in our days so perfected that it is not permissible for a medical man to allow a case of gonorrhœa to go on without curing it. There is no inflammation of the urethra which cannot be cured completely by appropriate treatment, but this result is only obtained by means of prolonged and painstaking observationc. Urethroscopy alone enables us to diagnose the lesions with accuracy and to apply the sovereign remedy.

7. Medical Research Committee, Special Report Series, No. 19, contains the following recommendations.

(a) After the patient has held his water for four to six hours and his meatus has been cleansed with alcohol, an attempt should be made to express secretion from the deeper part of his urethra. If this be present, films should be made.

(b) The prostate, vesicles and Cowper's glands should be massaged and films made and examined of any secretion which appears at the meatus.

(c) The urine should then be passed and the first ounce collected in a sterile tube. This is centrifuged and the deposit examined, smears and culture being made. The above procedure is of diagnostic value only when a positive result is obtained. A negative does not eliminate the possibility that the gonococci are still in existence in other regions of the body, nay, even in the regions examined by the bacteriologist. The gonococci, it is known, may lie latent in epithelial cells and while present, may not be actively discharged at the period when the examination is made.

It will be seen that there are variations in these tests of cure. Some require technique possessed only by a trained pathologist, which cannot be carried out by the ordinary practitioner. Others do not make sufficient attempts to stir up the gonococci which may lie latent in the genito-urinary tract.

It seems to the writer that something more uniform as to a standard of cure should be in general use and the following is proposed.

(1) On gently squeezing the anterior urethra after the patient has held his water for six hours, there should be no visible discharge or if any is present, it should be merely mucinous and contain no gonococci as demonstrated when a film is made and stained by Gram's method.

(2) A careful examination should be made of the meatus and glands, especially in cases of hypospadias, to see that there are no small discharging para-urethral sinuses. If any are present, slides must be taken of their secretions, stained and examined.

(3) The urethra should have been completely dilated by the passage of curved sounds.

(4) The patient should have no Cowperitis.

(5) The expression obtained at the meatus after massage of the vesicles and prostate should be normal to the naked eye and the urine passed by the patient after this examination should be free from pus and gonococci when centrifuged.

(6) The urethra should be normal throughout on urethroscopic examination.

(7) The complement fixation test should be negative.

(8) The patient should receive a provocative gonococcal vaccine of 100,000,000 and the condition of his urethral canal should remain unaltered for six days after this.

Taking these points one by one I may add that:—

The amount of discharge appearing at the meatus in the morning is not necessarily of great significance in judging whether the patient is still harbouring gonococci or not. Patients who have been receiving instrumental treatment over any lengthy period, may show for weeks afterwards large mucinous discharges, which on examination are found to contain abundant secondary organisms. The treatment for these mucoid discharges is avoidance of all mechanical irritation, for they diminish if left alone, provided the patient has come up to the standard of cure.

Para-urethral sinuses are usually easily found and should always be explored by probing and injected with some irritant silver solution to ascertain whether they may be harbouring the gonococcus or not. The writer uses injections of electrargol and takes a smear 36 hours afterwards, if necessary.

In average cases the urethra must permit the passage of a curved sound (size 24/28). This is necessary before urethroscopic examination is undertaken. If this has not been done, it is impossible to be sure that there are no patches of submucous infiltration harbouring the gonococci. In certain not uncommon

cases which have been treated by strong astringent irrigations or injections, hard infiltrations in the anterior 12.5 cm. of the urethra develope. It is sufficient if this infiltration is dilated by straight sounds so that a urethroscopic tube (size 48) may be passed through it into the posterior urethra.

By no Cowperitis is meant that Cowper's glands cannot be felt by the bi-digital method, but if they are palpable they should not be tender and should have remained unchanged in size after at least four examinations extending over a month.

It is very important to be certain that the vesicles are draining properly, otherwise erroneous conclusions are liable to be drawn from the naked eye examination of their contents appearing at the meatus after massage and from the centrifugalized urine. For this reason it is the custom of the writer always to obtain the vesicular and prostatic secretion immediately after the passage of a small, curved sound. If the vesicles feel hard and blocked on palpation and very little of their contents can be obtained after massage, a larger sound should be passed four days later and the examination repeated. After several dilatations, it is usually found that large turbid clots are expressed; a few days later it will be found on rectal examination that the vesicles feel more nearly normal. The writer has found that in cases of blocked vesicles the complement fixation test is always strongly positive. Within a month of the removal of the large clots, it becomes negative.

It may also be mentioned that the general condition of the patient very rapidly improves after the removal of this focus of infection.

No gonococci should be found in the urine passed by the patient after this examination, when centrifuged and stained.

The prostatic secretion is difficult to express alone, but can generally be obtained if the prostate is massaged correctly.

If the patient's urine is clear and on palpation there is no tenderness over the prostate, no active prostatic lesion is present.

In examining the posterior urethra with the urethroscope, an examination which should never be omitted, there should be no soft infiltration in any part. It is necessary to take particular note of the roof of this portion of the urethra above and behind the *verumontanum*. It is frequently found covered with small cysts or patches of soft infiltration, indicating infection of the anterior portion of the prostate. It is only by posterior urethroscopic examination that this source of infection can be excluded. There should be no polypi in this portion of the urethra and the *verumontanum* must be smooth and firm. If the openings of the ejaculatory ducts are inflamed, pouting or surrounded by polypi, further examination of the vesicles is necessary.

If the *utriculus* is gaping, it should be injected by means of a Geraghty syringe with some silver solution. (The writer prefers electrargol.) If the *utriculus* is an infected focus, gonococci will be demonstrable in the discharge provoked by this procedure.

There must be no discharging sinuses opening into the membranous urethra. Sometimes these are seen opening on the roof in the posterior portion and sometimes on the floor anteriorly. There must be no patches of soft infiltration in the anterior urethra. If at the bulb, these will frequently be found to be caused by vesiculitis or Cowperitis and if nearer the meatus, are generally found surrounding infected Littré's gland. The mouths of all patent glands should be injected with silver solution through the urethroscope and the urethral discharge examined for gonococci 36 hours afterwards. It is very necessary to carry out this provocative injection thoroughly, as the writer has frequently found small openings which could be injected with 1 c.cm. of electrargol or more. In suspicious cases all the glands should be injected on more than one occasion. It is only by this treatment that infected intra-urethral sinuses can be definitely excluded.

No patient should be permitted to marry unless his serum yield a negative response to the deviation of complement test. He may be passed as cured and considered up to the standard if a blood test which was formerly strongly positive, is later found to be very slightly positive, the patient having made some definite clinical improvement in the interval, for it will only be a matter of a short period before the blood test will become negative.

If the patient has been receiving vaccine treatment for his gonorrhœa, the complement fixation test will be influenced by the amount he has received and by the period which has elapsed since the last dose.

In certain cases of chronic urethritis negative complement fixation tests are obtained, while gonococci can be found in the urethral discharge. The writer has in these cases nearly always found some infected focus in the anterior urethra.

The patient's urine in a "one glass" test must be clear, but may contain minute epithelial shreds and mucus which do not sink; these may be disregarded. All others are of pathological significance.

If four and six days after a provocative vaccine injection, there is no increase in the urethral discharge and on examination no gonococci are detected in smears, or in films made from the centrifuged deposit of the urine and the condition of the urine remains unaltered, the test is regarded to be negative.

The focus must be sought if gonococci appear in the urethral discharge or if there is an increase of shreds in the urine, or it becomes turbid.

If a patient has been passing turbid urine all the time while under treatment, but in all other points comes up to the above standard, he must not be passed as free from his gonococcal infection unless the cause for this turbidity has been definitely determined, for example, pyelitis or *bacillus coli communis* cystitis.

Complete Examination in a Case of Gleet.

The following are the steps to be taken in the examination of a case of gleet.

At the first visit slides should be prepared from the discharge from the meatus. A slide should also be prepared from the sediment of the urine after centrifugalization. In the next place the prostate and seminal vesicles should be massaged and a slide should be

prepared from the sediment of the urine. For this purpose it is necessary to instruct the patient to pass a small quantity of urine for the first examination. After the massage he should pass the rest. A curved sound is then passed and the urethra is irrigated with a solution of oxy-cyanide of mercury. Lastly, a blood test is carried out. By this examination the surgeon tries to ascertain whether the vesicles are affected or not.

At the second visit three or four days later the same procedure is adopted, with the exception that Cowper's glands are examined instead of the prostate and seminal vesicles.

At the third visit a slide is taken of the discharge from the meatus and of the sediment of the total quantity of urine after centrifugalization. A complete urethroscopical examination is carried out with local silver application and subsequent injection. A provocative injection of a vaccine may be given at this visit or later.

At the fourth visit slides are prepared from the discharge from the meatus and from the centrifuged urine. If there is any doubt concerning the result of any part of the previous examination, this part should be repeated. If gonococci are not found in any of the slides, the patient is regarded as free from infection.

Summary.

(1) To carry out the above tests it is necessary to keep the patient in hospital for a period of ten days. In private practice this would necessitate about six visits extending over two weeks.

(2) The complete examination can be carried out by the medical man himself, with the exception of the complement fixation test.

(3) When the patient attains this standard, it is justifiable to tell him that he is free from gonococcal infection.

(4) A patient with a first attack of gonorrhœa can without any difficulty be brought up to this standard. In chronic cases it takes much time and patience to achieve the same result.

(5) If some such standard of cure as the above were in general use, it would be possible to compare the results obtained by newer methods of treatment.

(6) If the examination be negative in all points, there is no possibility of gonococci lying dormant in the genito-urinary tract. The patient may marry without risk of relapse, or conveying infection to his wife.

Appendix.

Of 56 consecutive cases admitted to hospital for examination of gleet:—

(1) 2 cases were not completed.
(2) 25 cases proved to be non-infectious.
(3) 24 cases proved to be infectious.
(4) 5 cases proved to be N.A.D..

The focus of infection in the 24 infectious cases were as follows:—

(1) Vesiculitis	10
(2) Cowperitis	5
(3) Prostate and vesicles	3
(4) Prostatic fossette	3
(5) Utriculitis	2
(6) Anterior urethra	1
	24

Reports of Cases.

CHOLECYSTITIS IN A PATIENT WITH TRANSPOSITION OF THE VISCERA.

By George Bell, M.B., Ch.M. (Syd.),
Honorary Assistant Surgeon, Sydney Hospital;
and
Roy C. Winn, M.B., Ch.M. (Syd.),
Formerly House Surgeon, Sydney Hospital.

Transposition of the viscera being of somewhat infrequent occurrence and acute disease of transposed organs less frequent, the following notes are of interest.

A.B., aged 37, a labourer, was admitted to hospital suffering from an acute condition of the abdomen on November 27, 1919.

He complained of severe pain in the upper part of the abdomen, more severe on the left side. The temperature was 38.9° C., the pulse and respiration rates 102 and 38 respectively.

The past history revealed no evidence of indigestion nor of any illness except gonorrhœa 25 years and syphilis 12 years ago. The patient stated that about 8 a.m. on November 26, 1919, while on his way to work, he was seized with severe pain in the "stomach" and indicated the left and upper part of abdomen and the left lumbar region as the site of this pain. He had not vomited and the bowels had been well open with aperients. About 2 p.m. on November 27, 1919, there was moderate abdominal distention and marked rigidity of the muscles of the upper segment of the abdomen, more particularly of the left rectus. There was also acute tenderness on pressure immediately below the left costal margin and over the upper part of the left rectus muscle. Liver dulness could not be demonstrated on the right side and the possibility of there being a perforation of some hollow viscus had been suggested. There was absolute dulness on percussion in the left nipple line, extending from the level of the sixth rib to one finger's breadth below the costal margin.

Examination of the chest showed the apex beat in the fourth right inter-costal space 10 cm. from the middle line and immediately beneath the right nipple. The question was then raised as to whether the heart was displaced by some cystic tumour or was transposed. Examination with X-rays by Dr. J. G. Edwards showed transposition of viscera, the apex of the heart being on the right side. A bismuth meal was given and the shadow of the stomach was also seen to be on the right side.

The leucocyte count revealed that there were 24,800 white corpuscles per cubic millimetre (polymorpho-nuclear leucocytes 90%, large lymphocytes, 5%, small lymphocytes 5%). The patient was not jaundiced.

A provisional diagnosis of acute cholecystitis or appendicitis in transposed organs was made.

Under general ether anæsthesia the abdomen was opened on November 27 by an incision through the upper portion of the left *rectus abdominis*. The greater portion of the liver was on the left side of the middle line. The fundus and body of the stomach were situated in the right hypochondriac and epigastric regions. The pylorus and the first part of the duodenum were lying to the left of the middle line and in the epigastrium. The round and falciform ligaments passed from the umbilical region upwards and to the left. The gall-bladder lay under the outer border of the upper part of the left *rectus abdominis*. It was acutely inflamed and distended and there was marked adjacent peritonitis.

A considerable amount of turbid fluid was aspirated from the gall-bladder and cholecystotomy performed. One large calculus, the size of a large olive, and numerous small friable calculi were removed. A drainage tube was sutured into the gall-bladder and brought out through the abdominal incision and another tube inserted through the left lumbar region into the left kidney pouch. The abdominal incision was then closed.

On December 2, 1919, the temperature had fallen to normal; bile drained freely for the first time from the tube in the gall-bladder.

Bacillus coli communis was found in the culture of the fluid removed from the gall-bladder.

The drainage tube was removed on December 13, 1919, and the wound had healed by January 2, 1920.

The patient remained well and was to have been discharged to a convalescent home on January 20, but became ill again on January 19 and complained of severe pain in upper part of the abdomen on the left side. The pain became worse on the following day, the temperature rising to 39° C. and the pulse and respiration rates were 136 and 32 respectively.

Cholecystotmy was again performed for an acute cholecystitis and infection of the track of the anterior drainage tube used at the first operation. The temperature became normal on January 22, 1920, and after this date the highest temperature registered was 37.8° C.. I am indebted to Dr. Steer Bowker for his assistance and advice at this second operation.

Subsequent examination showed that the patient was right handed and that the right testis hung lower than the left.

The following remarks by John McCrae, of McGill University, in the *Journal of Anatomy and Physiology* (1906, Vol. XL.), in recording a case in which transposition of all the viscera was complete with the exception of the gall-bladder, are of interest.

In his case the greater part of the liver appeared on the left side, but the gall-bladder lay to the right of the umbilical vein and suspensory ligament of the organ.

> Transposition of viscera is a comparatively frequently noted phenomenon. Arneill in 1902 stated that probably more than 300 cases are on record; many are observed during life and are not recorded. Thirty-three men, mostly clinicians, in answer to Arneill's inquiries, had seen 37 cases of which 31 were observed in life.
>
> Transposition of both abdominal and thoracic organs is the rule, Gruber finding it so in 71 out of 79 cases; of the eight cases where the abdominal organs only were transposed, all were incomplete.
>
> Steinhauser states that there is often some irregularity of the organs in abdominal transposition.

As regards the ætiology of this condition little is known. Adami sums up the position as follows:—

> Transposition of viscera naturally only shows itself in connexion with viscera that are not paired or do not occupy the median line: the heart and aorta, the stomach and intestines (organs which originating in the median line, with development become diverted in one or other directions), the spleen and the liver. To this statement there is one slight exception, *viz.*, the lungs which paired, exhibit different location on the two sides.
>
> There is only one known functional exception, *viz.*, the speech centres in the island of Reil, normally it would seem that the left set of centres is functional, the right latent; this may be reversed.
>
> The transposition may, on the one hand, affect only a single organ or group of organs, or, on the other hand, there may be complete *situs inversus*. Thus, the heart alone may be transposed, or the transposition may affect only the main arteries, the aorta passing from the right, the pulmonary artery from the left ventricle; or the thoracic organs may be normal, while the liver, spleen and viscera exhibit transposition. Evidently those partial cases can only be ascribed to local alterations in development. With regard to complete *situs inversus*, it has been put forward that the individual presenting the condition has been one of a monochorial twin pregnancy, that, derived from the longitudinal division of a single ovum he becomes a complete reflection, as it were, of his twin brother; where no history of twin birth can be obtained, it is suggested that the other brother becomes a *fœtus acardiacus* or *papyraceus*. Undoubtedly there are facts telling in favour of this view. For example, in not a few cases of reduplication by cleavage and superior dichotomy the organs of one half of the upper portion of the monster are transposed as compared with those of the other. But, on the other hand, the general rule is that monochorial twins present no sign of such reflection. In the majority of cases no indication is afforded of the existence of *situs inversus* in one of the two, while, conversely, Küchenmeister, studying 152 cases of transposition, found the history of twin birth in but a single case. A more likely suggestion is that the main current of blood to or from the germinal area becomes diverted at an early stage of existence, and thus purely mechanical influences lead the vessels of one side of the organism to receive more blood and therefore to grow more vigorously than those of the other. But it has to be confessed that we are still without any confidence regarding these hypotheses.

Carl Beck records a case of cholecystotomy in which, owing to transposition of the viscera, the incision was made through the left rectus.

Literature.

John McCrae, *Journal of Anatomy and Physiology*, Vol. 40, 1906.

Arneill, *American Journal of Medical Science*, Nov., 1902.

Adami, "Principles of Pathology," 1910.

Carl Beck, *Annals of Surgery*, Vol. XXIX., p. 593.

Reviews.

THE AUSTRALIAN NURSE.

Sister Anne Donnell has been well advised to publish her "Letters of an Australian Army Sister," which should be of interest to a wide circle of readers.[1] War from the point of view of the warrior can be studied in countless volumes, but it is a new sensation to peruse a record of the daily thoughts of a sensitive and highly observent woman during nearly four years of military nursing.

Miss Donnell sailed in June, 1915, as one of the staff of the 3rd Australian General Hospital and saw service in Alexandria, Lemnos and Cairo till September, 1916, when she was transferred to England Thereafter she was posted to many different hospitals in England and France, returning to Australia early in 1919. Throughout this long period she seems to have kept a careful diary in which she recorded her experiences and many of her thoughts especially in times of special stress. From this diary from time to time she transcribed in the form of circular letters such portions as she thought would be of interest to her intimate circle of friends and these letters, obviously unedited, have now been collected into book form. She writes very simply and naturally and is evidently very keen, appreciative and essentially womanly. Her devotion to her work is obvious and it can well be believed that the "diggers" who passed through her gentle hands, attached to her their "dinkum" label. But she also knew how to make the most of her time off duty and there must be few indeed who succeeded in seeing more of the people and places in the various localities she visited. What she saw she has very clearly sketched for her friends, so that her readers will find themselves constantly carried off from the sad and often gruesome hospital scenes to peep at mosques or ancient monuments in Egypt, Scottish hills, Irish towns or the sights of London.

The most thrilling part of the book deals with the notorious period when the cowardly Huns amused themselves night after night by dropping bombs over a large area set apart for hospitals in France. Writing at the time Miss Donnell indulges in no heroics, but very frankly describes her terror and that of her chums and describes how each tried to cheer the other up and all ended by sleeping under their beds. Yet it is quite evident that, on duty, these fears were well concealed and that the plucky girls went to and fro among their charges, cheering them and making fun of the whole tragedy.

No one, after reading these letters, will be inclined to dispute the claims of the Australian nurse to a position beside the "digger" on his pedestal.

AN INSTITUTE FOR MEDICAL RESEARCH.

A committee has recently been formed in connexion with the Royal North Shore Hospital, Sydney, to consider a scheme for the establishment of an institute for medical research. We understand that the promoters are about to appeal to the public for £30,000 to enable them to establish an institution on a site adjoining the Hospital.

[1] Letters of an Australian Army Sister, by Anne Donnell, of the Third Australian General Hospital; 1920. Sydney: Angus & Robertson, Ltd.; Crown 8vo., pp. 291. Price, 6s..

The Medical Journal of Australia.

SATURDAY, APRIL 10, 1920.

An Appeal to Those Who Served.

The story of the development of the medical branch of the Australian War Records Section is probably not known to the majority of those who served in the medical services of the Navy and Army during the war. It is scarcely realized by many how the officials connected with this branch have been striving to recover the time lost by the delay in its establishment. On May 12, 1917, nearly two years and nine months after the commencement of the war, Captain J. L. Treloar was appointed Officer in Command of the Australian War Records Section. Four days later he entered upon his duties in Chancery Lane, in offices little suited to the task and with a staff wholly incommensurate with the magnitude of the work to be undertaken. His duties were defined in simple language. They were to take over the diaries of the officers and others serving with the Australian troops in triplicate. In the course of four months it was recognized that the collecting of records and the preparation of the material for publication must be kept distinct, if anything approaching a reliable and accurate account of the war was to be brought to the light. About this time the responsibility for the collection of war trophies, of photographs, of military magazines, of sketches by members of the Australian Imperial Force, of personal *mémoires* and other souvenirs of historical interest and for their preservation was transferred from the office of the High Commissioner to the Australian War Records Section. It was nearly the end of the year 1917 when the scope of the Section was extended to cover Egypt and the East. By the appointment of Lieutenant Gullett a start, albeit late in the day, was made in Egypt. It will thus be seen that for over three long years, years crowded with incidents of military history of supreme importance to the world, years full of deeds of heroism and of unequalled devotion to duty, years of varying fortune of victories and of threatening disaster, years in which Australia won her laurels and established herself among the great nations of the earth—that during all this time no steps had been taken to record the history our men had been making. The idea of a medical history of the war, as far as Australia was concerned, was not born until the end of 1917. In November the plan was conceived and an attempt was made to carry it into effect. In December Colonel Graham Butler, D.S.O., was appointed to the office of Official Collator, while Captain Drummond was appointed Assistant Collator. Notwithstanding the stupendous task asked of the Collator, to trace and recover diaries, records, personal experiences and other evidence of events of the war during the three years and more prior to his appointment, as well as to continue to collect and collate the material as the armies moved slowly but surely toward the final *débâcle* of the enemy, this officer was not relieved of his ordinary arduous military duties. It would be superfluous to trace in this place the gradual expansion of the medical branch of the War Records Section, to follow the work as it increased day by day, week by week and month by month, steadily gaining in completeness and in importance. Our readers have had the opportunity of learning how fresh chapters were opened to the band of earnest workers and how new avenues of information were sought and found by dint of great perseverance and determination. Colonel Butler, Captain A. L. Maclean, Colonel R. M. Downes and others associated with the work sharpened their wits and achieved success to a remarkable degree, but they soon found that the chain of evidence had many weak links and that these deficiencies would have to be remedied before the records could be handed over to an historian to be moulded into a satisfactory shape. We have appealed from time to time to the medical officers of the Australian Army Medical Corps who served in Egypt, in Gallipoli, in Palestine, in France during the earlier periods of the war and more especially prior to the end of 1916, to send to the Collator their diaries, records and experiences, in order that the gaps in the story might be filled.

The Australian Medical History of the War is about to be written. The Official Collator, Major J. T. Tait, is preparing to hand over the valuable collection of records and material to the historian. Steps are now being taken to create such facilities for the writers that their task may be as easy of accomplish-

ment as it can be made. The Collator is making a final endeavour to supplement the records of the early stages, so that the writers need not delay in their work while they search for facts and for documents of value. We appeal again to the medical officers, to the nurses and to the non-commissioned officers and men of the Australian Army Medical Corps to search their files and to scour their memories for records and to transfer the data to paper, including every detail remembered or noted. The language can be crude, for these records are to serve but as guiding lines for the historian. Each medical officer has received a communication or several from the Collator, asking for information concerning the events with which he was connected. It is not too late to supply records to fill in the gaps in the narrative. The history must be constructed on the basis of individual units and consequently the facts have to be collected from the men who served in each unit. In certain instances medical officers were invited to contribute articles on special subjects, often of a technical nature. The Collator exercised care in the selection of these men, so that no one would be asked to write on a subject who was not well qualified to do so. Many of the men to which this appeal has been made, have not yet complied. We make a final appeal to all to assist in the great work about to be begun. Men who have achieved the apparently impossible in the field, will not hesitate to add a small task to their accomplishments. The completeness of the great monument of the medical services depends on the response to this appeal. Let it be spontaneous and immediate.

THE LABORATORY AND THE CLINIC.

But a few years ago the laboratory played a very small part in the education and life's work of the medical practitioner. The chemist, the physicist, the physiologist and the pharmocologist worked as it were on the outside of medicine and those who toiled within, paid little attention to the doctrines evolved largely on theoretical considerations and on the practical application of experiments which had but a small counterpart in the happenings in the clinic. The birth of bacteriology was a sign of a pending liaison between the laboratory worker and the physician. Close upon its heels came the hæmatologist and accompanying him the immunologist and the biologist insinuated themselves into the every-day work of the practising doctor. Each year witnessed a greater interest in and a more profound understanding by the laboratory worker of the problems of clinical medicine and surgery. Soon the practitioner realized that he had much to learn from the laboratory and that, if he wished to earn and retain a reputation as a competent clinician, he needed to acquaint himself with some of the laboratory methods and to conduct some of the investigations that formerly belonged exclusively to the laboratory recluse. A further development took place. Many laboratory research workers found that the problems they were investigating called for combined laboratory and clinical study and they passed backwards and forwards from the bench to the bed-side in the search for truth. The march of events could not stop at this stage. A necessary result of this increasingly closer co-operation between the man in the laboratory and the man in the clinic was the evolution of what is now known as team work, the co-ordination by one man of the work of many who have specialized in particular branches of medical science. In the future chemical, bio-chemical, biological, bacteriological and physical examinations will be regarded as indispensable when a patient presents himself for advice and treatment to a medical practitioner. To provide for this, greater facilities and more numerous laboratories will be needed and larger numbers of men and women will have to become trained in laboratory methods.

There is at present a tendency to oppose the laboratory findings in connexion with cases of disease to the results of the clinical examination. Physicians and surgeons, in a large measure, still look doubtfully at the laboratory report and take a particular delight when the clinical diagnosis stands at variance with the information obtained from the microscope or the test tube. This attitude is particularly unfortunate, because it reveals a want of understanding of the laboratory worker's functions. The practitioner consulted by a patient is required to make a diagnosis, to determine the methods of treatment and to bear the whole responsibility for the conduct of the case.

If he is sufficiently well trained and educated to perform the duties for which he accepts payment, he must act as the collector of information and as the judge of the evidence. He cannot protect himself behind the man in the laboratory who has carried out one or two tests at his suggestion. There are times when he may not regard himself to be competent to evaluate the evidence and in these circumstances he should ask his laboratory colleague to consult with him, to ascertain all that can be learned of the patient's condition and to express an opinion as to the significance of the signs and changes detected. But even after this free interchange of views and combined search for the key stone of the diagnosis, he has to accept the responsibility or to hand the care of his patient to someone who is better equipped to discharge the functions of a practitioner. Nothing could be more futile than to send a specimen to the laboratory and to ask for a report, without at the same time submitting a full clinical account of the physical signs and symptoms. The clinician expects an expert interpretation of the laboratory findings when he submits a specimen. The expert cannot give an opinion worth receiving, unless he is at liberty to call for any excretion or secretion or other available material and unless he can associate his findings with clinical manifestations. It is at times asserted that experimental research yields a solution to a problem which is at variance with the experience of clinicians. This argument is used to prove the fallibility of laboratory work. Laboratory workers, like clinicians, make mistakes and give wrong interpretations to observed phenomena. But human error should not be attributed to alleged defects in the system. If an experiment is properly performed and adequate controls and safeguards against fallacious results are introduced, there can be no question concerning the truth of the result. The competence of the laboratory worker depends on his ability to conduct his experiments in such a manner that the results are unmistakable and that only one phenomenon is observed at a time. It frequently occurs that the laboratory worker is required to answer a very complex question, such as which bacterium is responsible for a pathological change, when the material offered contains many species. He may be asked whether a person is suffering from syphilis on the evidence of the power of the serum to deviate complement from a hæmolytic system. Unless he is allowed to plan his experiment in such a manner that the result can yield a definite reply to the question, it is waste of his time and of the patient's money to have a laboratory investigation carried out. The value of the laboratory worker to the clinician will become increasingly evident, as the two approach each other and join in the difficult task of endeavouring to wrest from a jealous nature the secrets she keeps so closely veiled. The lesson to be drawn is that the clinician should learn more concerning the science of the laboratory and seek its aid freely, but that he should remember that valuable information can only be obtained if the association is a close one and if every means are taken to prevent a misinterpretation of the facts.

THE PHYSICO-CHEMICAL MECHANISM OF DIABETES.

Attempts have been made in the past to ascertain the exact mechanism involved in the disturbance of the carbo-hydrate and protein metabolism in pancreatic diabetes. That there is a definite relationship between the amount of protein that is metabolized in the body and the amount of dextrose in the urine in depancreatized dogs has long since been established. On the other hand, very little evidence exists concerning the fate of sugar derived from carbohydrate and sugar derived from protein in its passage through the blood into the urine. The researches of F. M. Allen have failed to throw light on the actual nature of diabetes. Allen defined diabetes as a disorder of metabolism due to a modification of the internal secretion of the pancreas. He has, however, not indicated the nature of the alleged modification of the pancreatic hormone, nor the mechanism of the disorder of the metabolism. During the recent years a few facts have been accumulated in regard to the effect of the failure on the part of the organism to deal with dextrose after the regulating action of the pancreas has been removed. It has not been possible to ascertain the bio-chemical changes which lead to the elimination of pancreatic activity in diabetes, but some progress toward a better understanding of the pathology of diabetes has been registered as a result of studies of the chemistry of protein and carbohydrate metabolism. A little more light has been thrown on this extremely difficult and complex problem by Dr. Dwight M. Ervin.[1] This investigator has shown that in the condition of pancreatic diabetes little or no glycogen is formed from the ingested glucose. He points out that if glycogen is not synthesized from glucose, the latter accumulates in the blood and passes thence into the urine. Rosenthal and Grahan have shown that it is a function of glycogen in the liver to prevent fatty changes in the parenchyma. In

[1] *The Journal of Laboratory and Clinical Medicine*, December, 1919.

certain forms of poisoning associated with fatty degeneration of the liver, the supply of glycogen is restricted. These liver changes can be prevented by feeding with carbo-hydrates. It has been suggested that as glycogen disappears from the cell when fatty changes occur, fat and glycogen exist in the cell in inverse ratio to each other. The author proceeds to disprove this assumption. Fat is held in an emulsified state by some form of colloid. The power to hold fat in this condition is dependent on the hydrophilic nature of the colloid. He adduces experimental evidence to show that glycogen is a much better emulsifying agent than protein. It is resistent to the action of acids, alkalies and salts and holds more fat in emulsion than protein. An emulsion of fat with glycogen remains colourless with iodine, but when the emulsion is broken down, the fat immediately takes on the stain. Similarly the glycogen turns red only when the emulsified state is destroyed. The addition of acids, alkalies and salts leads to dehydration of the colloid and consequently to the loss of emulsifying power. It can be demonstrated that in phosphorus, arsenic, mercury or chloroform poisoning, the glycogen is hydrolyzed from the protein-glycogen-fat system, leaving the protein-fat emulsion, which is readily broken down. The fat then appears in coalescence and becomes visible to the naked eye. The author claims that in pancreatic diabetes the failure of glycogen leads to a similar state of affairs. Further he argues on his experimental evidence that just as in fevers, a hyperglycæmia arises as a result of an increase in the hydrogen ion concentration, so in mercury or chloroform poisoning or in diabetes the increasing hydrogen ion concentration exerts a catalytic power to shift the equilibrium point between glycogen and glucose. He explains the appearance of acetone, not by assuming that the fat molecule is no longer burned to carbon dioxide, but that so large an amount of fat is offered for oxidation that some of it remains partially burned and the products, oxybutyric acid and aceto-acetic acid, escape through the kidney. In diabetes no glycogen is formed and as a result, the fats are but indifferently emulsified. A high concentration of fats or soaps reach the cells and as some of this fat is imperfectly oxidized, acetone bodies make their appearance.

TUBERCULAR AND ASSOCIATED INFECTIONS.

In a lecture delivered at the Brompton Hospital,[1] Dr. H. Batty Shaw has indulged in some very plain speaking. His experience and the cautious manner in which he marshals his facts, theories and hypotheses command consideration and attention. In the first place he re-introduces the term consumption, but gives it a meaning which differs widely from that formerly associated with it. He accepts as a fact that tuberculosis is caused by the *Bacillus tuberculosis*. On the other hand, he challenges the assumption that the destructive changes in the lungs, the appearance of pus in the sputum, the occurrence of hæmoptysis are signs of tuberculosis pure and simple. To him these changes are due to the associated pyogenic bacteria, such as the staphylococci, the streptococci and the pneumococci. He distinguishes between a person suffering from pulmonary tuberculosis, *sui generis* and consumption. In the latter there are signs of a destructive process, with general poisoning, as evidenced by fever, sweating and emaciation. He holds strongly with those who claim that in Europe nearly every child is infected with tuberculosis. While some children reveal serious effects of the infection, the majority overcomes the attack and no clinical manifestation of tuberculosis is detected. Later in life many individuals fall a victim of the disease. He does not deny the possibility of a re-infection from without, but is strongly of the opinion that this explanation is rarely the true one. The pulmonary tuberculosis common in early adult life is, in his view, the result of a re-infection of the patient from his or her original focus, a re-infection from within. He admits that in many of the infections of childhood the bacillus is of the bovine type, while in the common adolescent infection, the human type is almost invariable. But he is not convinced that the bovine bacillus cannot undergo transmutation into the human type after prolonged vegetation in the human organism. Without giving any very definite reasons for his refusal to believe that chronic pulmonary tuberculosis is the result of a fresh infection, he claims to find support in the epidemiological and clinical facts. He recognizes very little danger to the community from infective material, save in childhood, but he would not discard the sputum flask. He objects strongly to any measures being taken that would curtail the liberty and the comfort of the patient.

In the next place Dr. Batty Shaw questions whether the modern treatment of pulmonary tuberculosis by open-air methods is productive of good results. He argues that the apparently satisfactory results of sanatorium treatment are referable to the exclusion of the consumptive from the treatment. As in childhood, so in adult life, many pure infections with the tubercle bacillus become spontaneously healed. He throws up the question whether the lower mortality in warm, dry climates is not due to a prevention of bronchial catarrh with its associated purulent infections. He quotes certain statistics which have been used to support the contention that the mortality is lowered by sanatorium treatment. In certain series of cases which he has been able to follow, the ultimate result revealed the sad story of a very high death-rate. After giving full consideration to all the attendant circumstances, he is inclined to the opinion that the most potent factor in the prognosis of pulmonary tuberculosis or even of consumption, is the patient's resistance. He quotes as one of the uncontrovertible facts that it is unsafe to prophesy how long a patient with pulmonary tuberculosis will live. There are certain signs which may be taken as indications for a prognosis, but he does not appear to attach much reliance on them. He recognizes the importance to the patient of the degree of virulence of the bacilli. This, he maintains, cannot be influenced. The amount of infection is also of significance. It is reasonable to endeavour to reduce the quantity of bacilli in the body, but he disapproves of imposing hardships on

[1] *The Lancet*, January 24, 1920.

the patient in the attempt. In regard to the possibility of influencing the degree of resistance of the patient, he speaks guardedly. He thinks that the use of tuberculin in early childhood might have some effect. It would be worth while to investigate this point. Vaccination against a secondary infecting agent might result in a protection of the tuberculous patient from what he terms consumption. This, too, is but a suggestion, requiring trial and proof. If the liability to catarrhal infections and to suppurative processes could be influenced, the patient would probably have a better chance He claims, however, that the failure of the sanatorium and of other forms of treatment of tuberculosis indatees that the most that can be done, is to enable the patient to fight his tuberculosis with the means residing in his body. He applauds the action of many practitioners in Great Britain who persistently ignore the requirement of the *National Insurance Act* to notify tubercular infections. He holds that the patient's comfort, his right to liberty of action, his right to earn his living and his right to "live with his own" must be respected.

THE COLLOIDAL GOLD REACTION.

In the year 1901, Zsigmondy noted that the precipitation of gold from colloidal solution by means of an electrolyte (sodium chloride) was inhibited by the presence of proteins and certain other substances in colloidal solution. Considerable light was thrown on the behaviour of colloidal solutions by means of the gold precipitation test. In 1912, Lange applied the test to the cerebro-spinal fluid and claimed that the proteins in the fluid of persons suffering from syphilitic and metasyphilitic affections of the central nervous system could be demonstrated by its means at a period when other indications were ill-defined. It was soon demonstrated that the test in this connexion was dependent on the presence of globulin. On the other hand the test was shown to reveal a phenomenon different from the globulin test of Nonne. The latter test is performed by mixing cerebro-spinal fluid and a super-saturated solution of ammonium sulphate or as a ring test by floating the former on the latter. Since the introduction of the colloidal gold test, many criticisms have been published, some of which have aimed at the refusal of the test as a diagnostic means, while others have limited it to special laboratories on account of its alleged technical difficulties. During the past few years the essence of the test has been closely studied and the causes of irregular or paradoxical reactions have been analysed. Hitherto no one has been able to demonstrate a specificity of the precipitation of the gold, but attempts have been made to distinguish between the behaviour of the cerebro-spinal fluid in syphilitic conditions from that of the fluid in metasyphilitic and non-syphilitic lesions of the central nervous system. An interesting contribution to this somewhat complicated subject has recently been published by Dr. Margaret Warwick and Dr. Charles E. Nixon.[1] These workers have come to the conclusion that if the glass ware used in the preparation of the gold solution and in the test be cleansed with scrupulous chemical care and if the distilled water be prepared in a proper manner, the technique is by no means difficult and can be learned by any trained laboratory worker. They describe in detail the steps of the preparation for the test and of the test itself. In endeavouring to establish a definite significance of the test, they have studied the results of Nonne's globulin test, the cerebro-spinal cell count and the Wassermann test side by side with the gold test. Their patients were grouped according to the condition of the central nervous system. In the first place the cerebro-spinal fluid from persons suffering from general paralysis of the insane yielded a positive gold test more frequently than a positive Nonne test or a positive Wassermann test. The gold reaction revealed in fifteen out of eighteen cases a type spoken of as the paretic curve. This means that the reaction was positive in high concentration. A similar, but not so marked result was obtained in a series of cases of *tabes dorsalis*. In cerebro-spinal syphilis the gold reaction was positive with the maximum in less high concentrations in 77.79% of cases, while the Wassermann test of the cerebro-spinal fluid was positive in 48% and the Nonne test in 40.7%. On the other hand, in disseminated sclerosis and brain and spinal cord tumours the frequency of a positive reaction was lower and the type tended toward the third curve, in which the maximum was obtained with low concentrations. In meningitis a similar condition obtained, while in other affections the test was usually negative. From the results of their investigations they arrived at the conclusion that the colloidal gold test is more delicate than any other test applied to the cerebro-spinal fluid. They consider that it has considerable diagnostic significance when taken together with the other tests. It should not be regarded as a substitute for the other tests. The cerebro-spinal fluid of patients with no involvement of the central nervous system or of those who are not infected with syphilis, yields a negative reaction under all circumstances. They admit that the paretic or syphilitic curve at times occurs with the cerebro-spinal fluid of patients with non-syphilitic lesions of the central nervous system, but the differentiation can usually be made by means of the other tests. The colloidal gold test is unable to indicate the effect of treatment, nor does it vary in proportion to the improvement or otherwise of the clinical symptoms. From these findings it would seem as if the reaction were an extremely delicate test for the presence of globulin in the cerebro-spinal fluid as well as of other proteins. Since it is doubtful whether the presence of globulin in the spinal fluid is indicative of syphilitic infection involving the central nervous system, both this test and the ammonium sulphate test have a limited significance. When associated with definite signs of a syphilitic infection, they are strongly suggestive of an early involvement of the central nervous system and should be regarded as a warning to apply intensive treatment before it is too late.

[1] *Archives of Internal Medicine*, February 16, 1920.

The Council of the Victorian Branch has elected Dr. L. S. Latham to fill the vacancy on the Council created by the departure of Dr. T. P. Dunhill, C.M.G., from Australia.

Abstracts from Current Medical Literature.

MEDICINE.

(126) Diagnosis of the Anæmias.

A. H. Sanford states that more cases of pernicious anæmia are seen now than ten or fifteen years ago (*New York State Journ. Medicine*, December, 1919). In the differentiation of diseases in patients who complain chiefly of weakness, who have anæmia of obscure origin, with colour indices of about or slightly above one, with icteric tinges of the skin, or histories of slight chronic jaundice, with spleens that may or not be palpable, there are diagnostic tests of real value. (*i.*) Analysis of the gastric contents by a test meal and X-ray examination of the stomach; (*ii.*) a neurological examination and examination of the eye-grounds; (*iii.*) tests for the fragility of the erythrocytes; (*iv.*) detection of evidence of blood destruction by determinations for urobilin and urobilinogen in the stool, or, preferably, in the duodenal contents; (*v.*) serological tests for lues; (*vi*) examination of stools for parasites; (*vii.*) inspection of the mouth, especially the tongue, teeth and tonsils. The possibility of chronic sepsis originating here and of foci of infection elsewhere should be considered. In pernicious anæmia there are no free acids present in the stomach in nearly every case; the total acidity amounts to 6 or 8. X-ray examination of the stomach should be carried out to exclude malignant disease as a cause of the achlorhydria. The nervous symptoms resemble *tabes dorsalis*, but the changes in the cord are not only in the posterior columns, but in the lateral bundles as well a subacute combined sclerosis. In the medullary portions of the cerebrum there are changes similar to those found in the posterior and lateral funiculi. Subacute combined sclerosis is also found in leukæmia, Addison's disease, severe secondary anæmia, nephritis, pellagra, tuberculosis, syphilis, chronic alcoholism, lead, phosphorus or arsenic poisoning. In pernicious anæmia, unlike *tabes*, the reflexes may often be increased. Ordinarily Babinski's phenomenon and increased patellar reflexes are present. The gait may be ataxic, though later, unlike *tabes*, it may be ataxic and spastic. In severe anæmias, multiple hæmorrhages and small white degenerative foci are observed in the retina, particularly in progressive pernicious anæmia. Similar pictures occur in the anæmia due to anchylostoma and bothriocephalus. The retinitis in malignant cachexia and in all forms of chronic intoxications is very similar. It may be difficult to differentiate between pernicious anæmia and hæmolytic jaundice, which is characterized by anæmia. The laboratory test for the degree of resistance of the corpuscles in hypotonic salt solution may be an aid in the differentiation. Fragility is increased in jaundice of the hæmolytic type. In pernicious anæmia, as in the obstructive type of jaundice, either the erythrocytes are normal in their relation to hypotonic salt solution, or there may be an increase in resistance rather than in fragility. The hook worm and *Bothriocephalus latus* cause the symptoms of pernicious anæmia and should be looked for. *Endamœba histolytica* and *Balantidium coli* may also be associated with the symptoms of pernicious anæmia. In pernicious anæmia the tongue presents a peculiar glossitis. The absence of papillæ gives rise to the condition of "bald" tongue. Splenic anæmia of the child or young adult is evidenced by a secondary anæmia, leucopenia, large spleen and hæmorrhage from the stomach. It has to be distinguished from malaria with splenomegaly. Pseudo-leukæmia of infants or von Jaksch's disease may be a manifestation of this disease, though there is a leucocytosis. Gaucher's disease is easily separated from pernicious anæmia. In it there is a large-cell splenomegaly, with secondary anæmia.

(127) Angina Pectoris.

H. C. Gordinier contributes a comprehensive article on *angina pectoris*, embracing an historical survey and a description of the pathology, heredity, symptoms and signs, theories of the causation of the pain, prognosis and treatment of angina (*Medical Record*, October 4, 1919). In the inorganic or functional type there are no naked-eye changes and probably is caused by suddenly increased intra-aortic pressure from general vasomotor spasm. Cases of angina which are fatal, usually show associated changes in the aorta-coronary area, the supra-sigmoid part of the arch, the coronary arteries, myocardium and general vascular tree. In many fatal cases there are syphilitic changes in the myocardium. These are often secondary to changes in the coronary artery and may be due to a diminution of the blood supply to various areas of the myocardium. Most of the writer's patients were between the ages of 45 and 65. The earliest cases of "anginoid" pain observed by him were in girls at 8 and 13 years of age and a boy of 9, all of whom were suffering from basal pericarditis. Women are very much less frequently affected than men. Heredity undoubtedly exercises a prominent influence. Certain families are prone to degenerative vascular changes, both general and local. The writer has seen angina in brother and sisters, in two brothers, in father and son and in mother and daughter. A pain, be it mild or severe, referred to the thorax and excited by exercise against a resistance, is nearly always *angina pectoris*. Occasionally the pain starts in one or the other testicle or in a lower extremity. In rare instances the pain begins about the elbow and radiates to the præcordial region. Not infrequently it begins in the upper part of the abdomen and proceeds to the chest or it spreads in a reverse order. It may be confined to the upper part of the abdomen, when it is known as *angina abdominalis*. The systolic blood-pressure at the onset of an attack nearly always rises beyond the normal level found in the interval between the paroxysms. It may, however, remain normal or drop below normal. Angina is rare in sufferers with well-marked mitral regurgitation. The explanation probably lies in its effect in preventing high aortic pressure. In treatment, if relief does not speedily follow the giving of nitrites, recourse should at once be had to morphine and atropine. The latter drug will also help to stave off vagus inhibition. Additional doses of atropine should always be used if faintness, marked slowness of the pulse or an intense feeling of impending death should come on, for it is generally believed that these sensations are wholly or in part due to vagus inhibition. Atropine dilates the peripheral vessels and probably the coronary arteries.

(128) Pellagra.

E. H. Williams and G. H. Hunter relate their experiences of pellagra in Los Angeles and describe three cases in detail (*Medical Record*, September 20, 1919). Practically all of their patients developed profound mental symptoms, which terminated either in death or permanent dementia. At one time it was thought that the absence of pepsin and free hydrochloric acid in the stomach was almost pathognomonic of pellagra, but the authors did not find this deficiency in gastric secretion to be a constant factor. The writers differ from Yarbrough, who contended that the presence of burning sensations of the lower limbs and feet, general digestive derangement, tenderness over the abdominal area, general weakness without seeming cause, redness of the throat extending to a well-defined line, just above the pillars of the tonsils, a red, sleek, fissured or putty-coloured tongue and peculiar nervous symptoms warranted a positive diagnosis of pellagra, although there was no sign of dermatitis. In several of the cases nocturnal delusions were observed, or hallucinations and delusions, in which the patient wandered about somewhat dazed and confused, but did not express any particular type of dreams. Only about 40% of the patients complained of burning sensations in the lower limbs and less than 20% had dermatitis of the feet. All were adults and none had their feet or ankles exposed to the sun for any length of time. All of the patients were in comfortable circumstances, enabling them to have a diet satisfactory in quantity, quality and variety. Ross Snyder records a series of seven pellagrins in one family and three in another in the same neighbourhood. One family was living in dirt and filth. In the second family the children were kept scrupulously clean and the house and premises were well cared for. The one particular in which the cleanly pellagrinous children resembled their dirty neighbours was the fact that they did not eat meat, milk or eggs, not from necessity, but from choice. When these articles were added to their diet the symptoms of pellagra disappeared. Goldberger and his associates have concluded that the pellagra-producing diet-

ary fault might be corrected and the disease prevented by including in the diet an adequate supply of animal protein, particularly milk (including butter) and lean meat. It is possible that the disease is caused by some organism which finds suitable soil for growth in the systems of persons whose resistance is lowered by a diet lacking certain food elements. In any case, it is evident that diet is an important factor in many, perhaps all, cases of pellagra.

NEUROLOGY.

(129) The Vegetative Nervous System in Disease.

W. L. Brown (*Lancet*, May 17, 1919) in a Croonian lecture classified the visceral or involuntary nervous system as (*i.*) the sympathetic (thoracico-lumbar outflow), (*ii.*) the para-sympathetic: (*a*) cranial outflow, (*b*) sacral outflow. Just as the sensori-motor nerves are designed for localized accurate reflexes, so the visceral nerves are for widespread diffuse effects. Sympathetic stimulation serves to activate the body for a struggle and increase its powers of defence; and since the mechanisms for self-defence possessed at the present time were developing during vast periods of time, it is to be supposed that some are not suited to man's present needs; for example, the pilo-motor fibres. Some emotional responses, like some of our bodily structures, are vestigial remains. The function of the cranial portion of the para-sympathetic may be regarded as anabolic, while the sacral division, which consists of the pelvic visceral nerve, is mainly a mechanism for emptying. It was Gaskell who pointed out that the sympathetic and para-sympathetic are antagonistic. In pain, fear, rage and intense excitement the sympathetic neurons are brought rapidly into play, while the para-sympathetic are inhibited. Phylogenetically and functionally the adrenals and the sympathetic are in the closest association. The sympathetic excites a secretion of adrenalin, and adrenalin increases the sensitiveness of the response to the sympathetic. Emptying of the adrenal reservoirs occurs, not only in direct stimulation of the splanchnics and in strong emotions, but also in trauma and acute infections. This mobilization of adrenalin lowers the threshold to sympathetic stimuli, increases the blood sugar, hastens the clotting of blood by increasing the amount of prothrombin and diminishes muscular fatigue. The thyroid gland also has close relations with the sympathetic nervous system. The main thesis of the lecture is that stimulation of the sympathetic and its coadjutors, the adrenals and the thyroid, means the spending of reserves in the supreme struggle for existence.

(130) The Soldier's Dread of Danger.

René Cruchet (*Mercure de France*, 1919) discusses from the psychological standpoint the subject of fear in relation to the dangers of warfare. He begins by pointing out that fear is but an emotional reflex, one which necessitates the recognition of a sensation, especially a disagreeable or painful sensation. The physical phenomena of fear, the change of expression, the acceleration of pulse and respiration, the altered voice, etc., the intense lassitude and troubled sleep after the danger is past, all these when analysed may be shown also to be purely reflex reactions. Before going into battle, dread, accompanied by more or less anxiety, is the primordial phenomenon. The dread is not so much of death as of being wounded, or taken prisoner, or separated from comrades. On closer inquiry it may be shown that the fundamental disquietening idea is the dread of the unknown. Behind the reserve of some, behind the vivacity, even the exuberant gaiety of others, this apprehensiveness for unknown happenings can always be discerned. This emotional reaction is based on false preconceptions. Man after man, returned from his baptism of fire, has exclaimed: "I thought it would be quite different," or words to that effect. The dangers of the front are as nothing to the waiting hours when the imagination runs riot. In the actual presence of danger the element of surprise is of the highest importance and it may operate not only individually but collectively, so inducing panic. It is not so much the intensity of the danger which dominates, as the surprise it may occasion. Many a brave man who has often faced perilous circumstances with perfect *sangfroid*, has subsequently lost self-control in the presence of comparatively insignificant danger. The raw soldier is apt to underestimate danger and sacrifice prudence, the experienced man to err in the opposite direction. In a final word concerning surprise, Cruchet affirms that the French are successfully resistive against it, because it is part of their training and part of their mentality to be surprised at nothing. Numerous cases are quoted in support of these conclusions and though based on observations in the French army, they apply in a general way to troops of other nations.

(131) Reflex Phenomena in Spinal Injury in Man.

T. Graham Brown and R. M. Stewart (*Journ. Royal Army Med. Corps*, June, 1919) give a detailed account of the reflex phenomena encountered in cases of spinal injury in man. The majority of these phenomena have been observed previously in other mammals, under experimental conditions, but many of them, as they occur in man, are described here for the first time. This does not mean that they are of rare occurrence in the human subject, but that they have probably not been systematically sought for. The main interest in the paper is that these newly-observed phenomena show that the limb reflexes of man are strictly comparable in their various characteristics to the limb reflexes of other vertebrates. Indeed, apart from the phenomena, which can only be seen when the movements of isolated muscles are graphically recorded in the physiological laboratory, almost all the characteristics and variations of the vertebrate limb reflex have been observed by the writers in man.

(132) A National Institute of Neurology.

Harvey Cushing (*Amer. Journ. of Insanity*, October, 1919), pointing to experience gained in the war, emphasizes the immense value to neurology of bringing together those subdivisions which specialization has served to keep in more or less isolation. The need of a combination of neurologists, psychiatrists and neuro-surgeons, to continue with the care of damaged patients, is apparent, for the injuries of the nervous system will be the last to remain in military hospitals and they are admittedly the most difficult of all to treat, whether the demands are for surgical or psycho-therapeutic measures. But this fusion of neurological interests is only an immediate programme. Out of it there should grow a permanent institution, to serve as a future national centre of neurological interests. Its appointees should be on a full-time basis as teachers and investigators. There should be a wise director, with sub-directors for the departments of psychiatry, organic neurology, neurological surgery and so on. There should also be a great central laboratory. The organization would be comparable to the Queen's Square Hospital, in London, or the Salpêtrière, in Paris, but on a far broader scale.

(133) An Acute Prison Neurosis of the Anxiety Type.

N. S. Yawger (*Journ. of Nerv. and Ment. Dis.*, October, 1919) writes that one would expect that convicts entering upon penal servitude should at times show nervousness, but that they should become ill from the prospect of release is hardly within one's expectations. Yet this is the case and occasionally a more or less definite anxiety neurosis is seen. About one-third of those coming up for pardon or parole show this nervousness, which is jokingly called "pardonitis" or "parolitis." The symptoms are pre-eminently somatic. The subject shows an anxious expression, restlessness, decreased power of concentration, irritability, introspectiveness, disturbing dreams, insomnia, dyspepsia, genito-urinary disturbance, cardiac irregularity, loss of weight, a degree of tremor and increased tendon reflexes. This neurosis arises in constitutionally inferior individuals. Causally, while the convict commonly masturbates, it is impossible to determine how much responsibility is attributable to the sexual sphere. Among other factors, the prisoner's mind is beset by all kinds of reasons why he may not be released. A few know that objections have been lodged against them, which is disturbing, and all feel that when discharged they may be homeless and certainly will be unwelcome members of society, which, after serving their sentence, they feel is undeserved.

British Medical Association News.

SCIENTIFIC.

A meeting of the Victorian Branch was held on March 3, 1920, at the Medical Society Hall, East Melbourne, Dr. John Gordon, C.M.G., the Vice-President, in the chair.

The Vice-President welcomed the following members who had returned from active service since the last meeting of the Branch: Lieutenant-Colonel A. Cook, O.B.E., Lieutenant-Colonel C. Morlet, D.S.O., Captain A. H. Baldwin, Captain E. H. Britten, Captain H. G. Brown, Captain A. E. Burke-Gaffney, Captain J. F. Chambers, Captain J. R. Donaldson, Captain R. Fraser, Captain W. A. Graham, Captain F. McCallum, Captain M. McKenna, Captain H. McLorinan, Captain H. W. Savige, Captain W. F. Stephens and Captain J. R. Williams.

Mr. Victor Hurley read a paper on surgical shock (see page 331).

Mr. Hamilton Russell, in opening the discussion, said that although he did not feel fully qualified to comment upon the newer conception of surgical shock, as presented by Mr. Hurley, yet he experienced a real satisfaction in expressing his thanks to Mr. Hurley for such a valuable paper. The condition of surgical shock had been for long enough one of the greatest mysteries of practice and any light which could be thrown upon it, was of very great value. But, having in mind the facts that in shock the nervous system was largely involved and that knowledge of the workings of the nervous system was so slight, he would caution them against the danger of being guided in their treatment by the theory of the moment, rather than by the teachings of clinical observation. There was a very great danger in all questions in importing the lore of the physiological laboratory into the operating room. It was possible that efforts to put into practice half-assimilated physiological principles, might lead to disaster. Such an untoward result had attended the influence of purely laboratory studies upon the administration of chloroform years ago.

Mr. Russell referred to some remarks made by Dr. Stawell at the previous meeting of the Branch, on which occasion he had been very much interested to hear the view put forward that chemical substances, allied to histamine, might play an important part in the production of surgical shock. It followed from such a conception that one of the reasons for amputation of a torn and shattered limb was to cut off the supply of the noxious chemical products absorbed from the damaged tissues. This conformed to an early observation of Lister, which, if he remembered correctly, would be found in an article on anæsthesia in Holmes's "System of Surgery," that the amputation of an injured limb under chloroform was often attended by an improvement in the condition of the patient.

It was only a short time since the treatment of surgical shock resolved itself into large doses of strychnine and he well remembered when patients were sent back to bed and ordered 0.002 grm. of strychnine at longer or shorter intervals. He had always had qualms about this procedure and had felt that their knowledge was not accurate enough to warrant the use of such a powerful drug, in the case of patients so near to death. He was not surprised when it was shown later that the administration of strychnine was not only useless, but did actual harm by aggravating shock.

Mr. Russell remarked further that whatever might be the outcome of the present trend of research into the factors underlying surgical shock, he could not help retaining certain conclusions drawn from years of experience in surgical practice. War time was apt to show one side only and to present the most extreme degrees of shock. But in civil practice he had noted that where an operation was difficult, or performed badly with a good deal of mishandling, without any other particular reason, the patient suffered severely from shock, whereas a well-performed operation, even though lengthy, had not the same shock-producing effect. This was an observation for which he could not offer an adequate explanation in theory, but it was none the less his experience.

Then there was the condition which, if it had to be given a name, could be described only as chronic shock. It was produced by the repeated daily occurrence of painful experiences, as, for example, surgical dressings.

Crile had shown that the brain changes were the same in the severe type of shock dealt with by Mr. Hurley, as in the shock which followed repeated frights or painful dressings. All surgeons of experience must have seen these evil results of chronic shock. They might be typified in the case of a boy with extensive burns. After recovery from the "primary shock," the boy was under the necessity of undergoing painful dressings every day. At first he bore them well, but after a time he became unruly and increasingly difficult to dress. Later, even though the dressings would be done painlessly, the boy would scream at the mere suggestion. A similar condition was seen after railway accidents and it mattered not what measures were taken to alleviate the pain of necessary dressings and attentions, the patients would show every evidence of severe suffering and nervous stress.

Such a condition he had been accustomed to call "acute neurasthenia," but he was convinced that it had a relationship to surgical shock. As Crile had demonstrated, it was attended by identical changes in the central nervous system. In conclusion, Mr. Russell asked permission to repeat his expression of thanks to Mr. Hurley for a very interesting paper.

Dr. R. J. Bull expressed his appreciation of Mr. Hurley's masterly summary of present-day views and opinions regarding surgical shock. During the reading of Mr. Hurley's paper, it had occurred to him that the relation between surgical shock, or wound shock, and anaphylactic shock might be more real than apparent. Anaphylactic shock, of course, could be excited after sensitization of the organisms to foreign, and even homologous, proteins, in themselves innocuous. If wound shock and anaphylactic shock could be shown to be analogous, the fact would lend support to the idea that surgical shock rested on a chemical basis; whether the tissue damage in the latter condition was effected by actual injury, or by mere cutting off of the blood supply, it seemed quite feasible that the natural muscle ferments acting on the natural proteins might lead to the formation of cleavage products of a highly toxic nature.

In animals in a condition of anaphylactic shock, the blood serum showed a reduction of complement. Dr. Bull thought that the amount of complement present in the serum of patients suffering from surgical shock might be made the subject of investigation. If the complement could be shown to be consistently reduced, a point of analogy between surgical and anaphylactic shock might be established. Again, the sensitized state underlying anaphylactic shock could be passively transmitted from one animal to another. He would like to inquire of Mr. Hurley or any subsequent speaker, whether any observations were on record regarding the possibility of a similar transference of the condition of surgical shock. He realized that experimental results might be anomalous, unless in the very early stages of shock, as once anaphylactic shock had developed, the sensitized condition disappeared.

Dr. Bull remarked further that he had been very much interested by the groupings of donors for blood transfusion, but, at the same time, he thought that perfectly satisfactory tests could be carried out without the standard Group II. and Group III. sera. A small quantity of blood and a few drops of citrate suspension of the donor's and recipient's corpuscles were all that was necessary. After its separation the serum was tested against the citrate suspension of the donor's washed corpuscles and *vice versa* and any agglutination or hæmolysis observed. He had recently had occasion to carry out the test in the manner indicated and the result was quite satisfactory. The method could be recommended when it was inconvenient to carry out the test at the bedside.

Dr. S. F. Macdonald asked permission to refer to one point, although it was rather foreign to the discussion proceeding with regard to the factors at work in the production of surgical shock. Blood transfusion was now an easy procedure of almost every-day occurrence. Many were prepared to utilize it and he anticipated that sooner or later transfusion of blood would be given trial as a method of treatment in grave forms of anæmia. Indeed, it had been so tried in America and as far as his information went, with some bad results.

If blood transfusion of this nature should be carried out

in Australia, he would suggest that the hospital pathologists be asked to keep an exact record of hourly observations on the recipient's blood. He had learned of two cases in America in which the injection of blood seemed to provoke an entirely new form of phagocyte which engulfed not only the donor's corpuscles, but also the recipient's. One case only had came within his personal experience; in that instance the donor's corpuscles were promptly phagocyted.

Mr. Alan Newton congratulated Mr. Hurley on his excellent and concise summary of the present-day position regarding surgical shock. He wished to mention some experimental work carried out by Bazett, before the war, which seemed to supply an answer to one of the questions raised by Dr. Bull. The observer referred to eventually succeeded in switching the circulation of a shocked animal into that of a normal one and *vice versa*. No change could be observed in the normal animal as the result of its having received a quantity of blood from the circulation of the animal in a state of shock.

Bazett conducted further experiments with the object of determining the amount of blood lost during surgical operations. His general conclusion was that, as a rule, there is more blood lost than the surgeon usually believes.

Mr. Newton laid much stress on the great utility of gas and oxygen anæsthesia in obviating surgical shock. All who had worked in France were convinced of its value, yet, since his return, he had been surprised to find that little or no attempt was being made in Melbourne to use gas and oxygen as an anæsthetic. He had been met with the objections that it was too cumbersome and too expensive and with various other excuses. He thought that the British Medical Association should take steps to popularize gas and oxygen and should further urge the installation of the apparatus in the public hospitals. He had no doubt that many lives would be saved by the use of gas and oxygen, which would surely be lost if chloroform and ether were employed.

Dr. Frank Andrew agreed with Mr. Russell that there was a danger to be avoided in carrying half-digested ideas from the laboratory into practice. He had been very interested in the experimental results obtained with histamine, but were they to regard all shock as the product of absorption of histamine or similar bodies? He found it difficult to believe. They must all have seen profound shock supervene in patients where the only structures divided were skin and bone and where no blood had been lost. He felt that they could not ignore the effect of what Mr. Russell had termed "mishandling," bad anæsthesia, and bad mental environment in the production of shock. He would like a definite answer from the advocates of the bio-chemical basis for shock, as to whether "histamine" was the only factor concerned. If it were not so, then he felt very strongly that they should not lose sight of the mental preparation, skilled anæsthesia and other measures that might be taken to cut out the transmission to the nervous system of shock-producing impulses.

Dr. R. R. Stawell addressed himself to the points raised by Mr. Russell and Dr. Andrew. It should be understood that Professor Bayliss and those associated with him recognized two varieties of shock, *viz.*, (*i.*) primary shock, or collapse, of nervous or vaso-motor origin and different from (*ii.*) wound shock, profound and persisting. Not the most ardent bio-chemist would deny the initial nervous disturbance.

The essential condition in both forms of shock was a lowered blood pressure and consequent poor oxygenation of the tissues, a state of affairs which, if persistent, inevitably led to tissue damage from impaired nutrition.

Even when Bayliss's view—which seemed a definite advance in knowledge—was accepted, it did not seem that the "Crile" view of a nervous basis for the initial process, as opposed to persisting shock, could be altogether dismissed.

Mr. Hurley, in reply, expressed his thanks to the various speakers who had participated in the discussion. It gave him particular pleasure that Mr. Russell had opened the discussion. He had always regarded it as a special privilege to have had the opportunity, as a student, of attending Mr. Russell's clinic at the Alfred Hospital. Upon the occasion of the reading of his first paper since his return from war service, the presence of Mr. Russell and his kind remarks gave him the greatest pleasure.

With reference to the various aspects of the discussion, Dr. Stawell had dealt with some and Mr. Newton with others. Bazett and Crile had both failed to find transferability of shock. Regarding histamine, Bayliss did not claim that it was the actual substance incriminated in the production of wound shock, but only that some chemical product or products played such a part. Histamine had come into prominence as the nearest experimental parallel, but Bayliss did not insist that the actual agents were necessarily allied to histamine in their chemical nature. The most enthusiastic biochemists did not believe that all cases of shock were essentially chemical—far from it—but no doubt Bayliss unduly emphasized this aspect of the question.

As pointed out by Dale, in the conditions obtaining in surgical shock, *e.g.*, dilated capillaries, with constricted arterioles and escape of fluid into the tissues, a close approach was seen to the basic phenomena of ordinary acute inflammation. Recent work had resulted in a material advance, but further research was necessary.

Mr. Hurley pointed out the probability that early sepsis was a factor underlying the symptoms in many of the gas gangrene and bad wound cases. In the matter of blood transfusion for grave blood diseases, mentioned by Dr. Maedonald, he had had no experience.

A meeting of the Queensland Branch was held at the B.M.A. Rooms, Adelaide Street, Brisbane, on March 5, 1920, Sir David Hardie, the President, in the chair.

The President welcomed Captain A. P. Murphy on his return from active service.

Dr. J. Lockhart Gibson presented a case of hernia of the vitreous into the anterior chamber. The hernia was the result of a perforating wound of the cornea and iris, with prolapse of the eye. The accident had occurred six years previously. Four years later the patient had received a second blow on the eye. The present condition was the result of the repeated trauma. Dr. Lockhart Gibson at first considered it to be a dislocation of the lens, followed by a prolapse of the vitreous. The lens, however, was in its proper position. As the condition of the eye was not causing any trouble to the patient, he proposed to leave it alone.

Dr. R. Graham Brown expressed the opinion that it was a cystic condition, following a penetrating wound of the anterior chamber.

Dr. W. F. Taylor stated that he regarded the case as a very unusual one. It was the first of the kind he had seen.

In his reply, Dr. Lockhart Gibson said that there was no reason to suppose that the condition was cystic. The opacity was due to the hernia of the membrane through the opening. If anything were done, more membrane would prolapse through the rent.

Dr. V. N. B. Willis read a paper entitled "A Plea for a Standard of Cure in Cases of Gonorrhœal Urethritis in the Male" (see page 336).

Dr. V. McDowall congratulated Dr. Willis on his paper. He held that the standard of cure constantly changed as each new drug or method of treatment came into vogue. It was often found that the disease remained uncured, notwithstanding the fact that the bacteriological examination of the urine and the biological examination of the blood yielded negative results and the urethra, when viewed through the urethroscope, appeared to be normal. The speaker held that the only guide of value was clinical experience; and even clinical experience was not always reliable.

Dr. J. G. Avery agreed with Dr. Willis that a standard of cure for gonorrhœa was necessary, but he found it hard to attain. He considered that it was difficult to carry out posterior urethroscopy. Many of the conditions described by Dr. Willis were conspicuous by their absence on examination. Cowperitis was rare and was difficult to treat. The only treatment of value was massage and dilatation. He asked Dr. Willis how he treated dilated lacunæ in the anterior part of the urethra, after a silver salt had been injected. He found that the actual cautery was generally required to provide sufficient drainage. In his opinion cases in which a negative complement deviation test was obtained, while gonococci persisted in the discharge, were incurable.

Sir David Hardie said that while listening to Dr. Willis's paper he had wondered whether all the steps described were necessary. If they were necessary, the general practitioner would not be justified in attempting the management of these cases; he should refer the patients to a specialist.

Dr. Willis thanked the members for the manner in which they had received his paper. He said that he would vary the standard of cure, if a new, efficient method of treatment were introduced. The object of the standard was to discover new methods of treatment. Of 400 patients under his care who were passed as cured on the basis of the standard, only four relapses subsequently occurred. These relapses were due to the overlooking of some lesion, such as a Cowperitis. If the infection were cured in the early stages, there would be no need for the procedure he had described. He held that it was not difficult to examine the posterior part of the urethra. He also stated that Cowperitis was not a rare condition. In 50% of the chronic infections seen at Bulford, the condition was caused by Cowperitis. The openings of the para-urethral ducts should be cauterized; this provided free drainage. A negative blood test did not have any significance; a positive one alone was of diagnostic value.

Dr. R. Graham Brown and Dr. Ellen Kent Hughes read a paper on tonsillectomy as a means of treatment of diphtheria carriers. This paper will be published in a subsequent issue, together with a short summary of the discussion.

Dr. R. Graham Brown exhibited a man, aged 29 years, with chronic suppuration of all the nasal accessory sinuses. The patient had been subjected during the preceding 18 months to a sub-mucous resection of the nasal septum, a radical antrum operation on the left side and a partial double exenteration of the ethmoid bones. The patient was very ill when seen. A temporary resection of the nose was carried out through von Eiselsberg's incision. The antra were dealt with by the Cantfield-Ballenger procedure; both ethmoids were dealt with in a radical manner and both sphenoidal sinuses were drained. The frontal and sinus ducts were greatly enlarged. The result of these procedures was satisfactory, although there was still evidence of further disease. Very little deformity was seen. The skin had become fresh and rosy. The severe headaches of which the patient had complained, had disappeared. Dr. Graham Brown was of opinion that some further small operations would be required at intervals during the following year or two before the disease could be combated.

Dr. C. A. Thelander showed a pregnant uterus with multiple fibroids. The chief points of the case were as follows:—

A woman, aged 38 years, a nullipara, had suffered from severe menorrhagia for several years. There was a history of a three months' pregnancy. On vaginal examination a hard mass, the size of a fœtal head could be felt; the cervix was found lying posterior to the mass. Hysterectomy was performed. A three months' fœtus was found within the cavity of the uterus.

MEDICO-POLITICAL.

At the meeting of the Queensland Branch held on March 5, 1920 (see above), the subject of the medical curriculum was considered.

It was announced that the following had been elected members of the sub-committee appointed to consider the medical curriculum for the proposed medical school in the University of Brisbane: Dr. A. Jefferis Turner, Dr. J. Lockhart Gibson and Dr. E. Sandford Jackson, representing the English, the Scottish and the colonial schools respectively. Dr. Lockhart Gibson asked whether they had power to add younger men to the sub-committee. After discussion, it was decided to allow the matter to stand over for a time.

The President drew the attention of the members to the rules of the Branch in regard to advertising. He stated that some of these rules had been ignored by members.

The undermentioned have been elected as members of the Victorian Branch:—

John Wyndham Morgan, Esq., M.B. *et* Ch.B., Melb., 1911, Beulah.

Herbert Harold Spencer, Esq., M.B., Melbourne.

Vernon Carlisle Brown, Esq., M.B. *et* Ch.B., Melb., 1915, Children's Hospital, Carlton.

The undermentioned have been elected as members of the Queensland Branch:—

Edward Thomas Wynne, Esq., M.B., Camb., 1888, M.R.C.S., Eng., L.R.C.P., Lond., 1887.

Herbert Alfred de Pinna, Esq., M.R.C.S., Eng., L.R.C.P., Lond., 1905, Aramac.

The undermentioned have been nominated for election as members of the New South Wales Branch:—

Thomas Action, Esq., L.R.C.P., Lond., M.R.C.S., Eng., 1900, C/o. Bank of New South Wales, Head Office, Sydney.

Philipe de Luca, Esq., M.D., 1895, Univ. Naples, 22 Darlinghurst Road, Darlinghurst.

TUBERCULOSIS CAMPAIGN IN BENDIGO.

Arrangements have been made by the Federal Government and the State Government of Victoria to carry out the recommendations put forward by the Committee appointed by the Department of Trade and Customs to inquire into the causes of death and invalidity in January, 1917, in regard to the institution of a vigorous campaign in a limited area against tuberculosis. In the report issued by the Departmental Committee the recommendation was made that the Victorian Government might co-operate with the Commonwealth Government in such a campaign; the mining district of Bendigo was mentioned as a suitable field of activity. An executive committee has been formed with Mr. J. H. L. Cumpston, the Director of Quarantine, and Dr. E. Robertson, the Chairman of the Board of Health of Victoria as members. The control of the campaign is placed in the hands of Dr. D. G. Robertson, lately of the Quarantine Service. The objects of the campaign are:—

(*i.*) To determine the actual number of persons suffering from tuberculosis in Bendigo and district.

(*ii.*) To ascertain the epidemiological factors involved in connexion with the existing infections. These factors would include an investigation into the evidence of infection of persons who have been in contact with infected persons the relation of the spread of the disease to insanitary conditions, the relations of the disease to mining, etc.

(*iii.*) To determine the relative importance of bovine and of human infection in the distribution of the disease in Bendigo.

It is anticipated that the campaign will extend over a period of six months. It is intended to serve as a preliminary test. The experience gained will enable the authorities to determine the measures of prevention necessary to cope with the disease. Simultaneously a hookworm survey is to be carried out by the same staff of workers. The plan of work is being drawn up by Dr. Sawyer, the Director of the Australian Hookworm Campaign.

PREFERENCE TO RETURNED SOLDIERS

The following letter, which has been addressed by the Society of Returned Medical Officers of Queensland to the Under Secretary at the Home Secretary's Department, Brisbane, is self-explanatory. It enunciates a principle which has the whole-hearted support of the medical profession:—

Sir: I have the honour by direction to inform you that at the last meeting of the Council of the above Society I was instructed to write as follows:—

It is understood that it is the policy of the present Government to give preference to returned soldiers in all the appointments at their disposal. In view of this policy, this Society trusts that in the appointments to the honorary staff of the Brisbane Hospital about to be made, and other posts under their patronage, other things being equal, the returned soldier will be given preference, and in no case be penalized for his absence on active service.

Yours, etc.,

J. J. POWER,
Honorary Secretary.

March 26, 1920.

MEDICAL REGISTRATION IN VICTORIA.

Our attention has been directed to an error in the brief summary of the qualifications for registration published in our issue of March 27, 1920, page 298. *The Medical Act,*

1915, of Victoria, which superseded the Act of 1890, provides that persons who have passed through a course of medical study of not less than five years' duration in a foreign university, college or body duly recognized in that country, and who possess a diploma or degree entitling them to practice in that country, may be registered in Victoria. This provision is restricted by a "reciprocity" clause. One person holding the qualifications of the Boston Homœopathic University and College or of the New York Homœopathic Medical College and Hospital may be registered each year in Victoria.

Obituary.

FREDERICK JOHN ELLIOTT.

It is with much regret that we have to announce the death of Frederick John Elliott, of Derby, Western Australia, on December 3, 1919. He was the son of the late James Elliott and was born in the north of Scotland in April, 1855. He was educated in Scotland and studied medicine in London, having been a student at Guy's Hospital. In 1897 he qualified with the diploma of membership of the Royal College of Surgeons of England. He served a term as House Surgeon at his old hospital and in 1898 he came to Australia and was appointed to the position of Resident Medical Officer at the Townsville Hospital, in Queensland. A year later he started practice at Donnybrook in Western Australia. After a short time he was appointed district medical officer and Chairman of the Preston Road Board. He was greatly respected at Donnybrook and was a very successful practitioner. In 1914 he left the district, after having looked after the interests of the residents for fifteen years. In June, 1915, he was appointed district medical officer at Derby. He filled the position of magistrate here as well as at Donnybrook. After four and a half years of active life, he was suddenly struck down and died after a short illness.

Correspondence.

REFORM OF THE MEDICAL CURRICULUM.

Sir: In your issue of March 27 five signatories have put forward concrete proposals for a reform of the medical curriculum at the Sydney University. The proposals have evidently been carefully thought out and the effort is one to be commended. I find, however, there is some room for criticism. The natural sciences are to be added to the compulsory subjects for the entrance examination, which is quite right, but classics, which means in most cases compulsory Latin at a high standard, is to be retained, which is an evidence of conservatism one would hardly expect from reformers. I need not argue this point, for one can see by the action of other universities that compulsory Latin is dying a natural death—from old, very old, age. Next it is proposed to send the students to the hospitals during the second year. This, I think, would be a grave error. One reason urged is that the student would be able to see fractures and note the displacement caused by muscular action. Apart from the fact that one high authority has declared that muscular action does not cause displacement in fractures, the instance is not a good one, for in the first place, except to the trained eye, there is not much to be seen in most fractures and in the next a fracture in a living person means so much more than the mere break in the bone that a false, early impression is likely to be given to the student and even if this be not harmful to him, it certainly would be of no use. As to fracture, so to other morbid conditions. I believe that hospital work in the second year would do some harm to the student and would waste his time. Another striking thing in the proposed curriculum is the extraordinary amount of time to be given to chemistry. Omitting inorganic chemistry, pharmacology, materia medica, hæmatology and other subjects in which chemistry may come in, I find 290 hours suggested for special instruction in chemistry. There is applied chemistry, organic chemistry, bio-chemistry, physiological chemistry, chemistry of foods, pathological chemistry and chemistry of drugs and poisons. One would almost think that one of the objects of the signatories was to make research scholars in chemistry of every graduate. But to what purpose? The number of occasions on which a practitioner requires an elaborate chemical examination in his routine work is very few. Chemical physiology is in a state flux and opinions are constantly changing in regard to it; for instance, Haldane, one of the greatest and most practical of our physiological chemists, has recently recanted his view that hydrogen ions in the blood solely control respiratory activity and now claims that anoxæmia plays a very great part in the process. There is unlimited work to be done in bio-chemical research, but the present practical needs of the profession do not warrant a disproportionate amount of time being given to its study in the curriculum. Chemistry and physics are extremely important subjects and in their fundamentals should be very thoroughly taught, but in their more highly specialized branches and technical details must at present be left to research scholars and specialists.

The proposed curriculum makes no provisions for lectures on medical ethics, a matter of some importance. On the other hand one must approve of the attempt to coordinate the various teaching departments, the proposal to eliminate studies that do not bear on the ultimate object of the school and the suggestions for bridging the hiatus between the early lectures on anatomy and physiology and the later ones on their application to medicine and surgery. The proposal, however, to leave the anatomy and physiology of the nervous system and the special senses to the fourth and fifth years respectively needs some consideration. The nervous system especially is so bound up with general anatomy and physiology that the proposal to leave its considerations over to a later date must not be done lightly. In conclusion, sir, I would like to say that your signatories are to be applauded for applying themselves to such a difficult subject in so comprehensive a manner and that my criticism is meant to help in the construction, not in the destruction, of their proposals.

Yours, etc.,

W. F. LITCHFIELD.

March 29, 1920.

[Dr. Litchfield makes two fundamental errors in the first paragraph of his letter. The report is signed by six medical practitioners. The report contains recommendations for the reform of the medical curriculum in the Australian medical schools. No mention of any one university is made.—Ed.]

TOXÆMIA AND SEPSIS.

Sir: In the sub-leader of the *Journal* of March 13 dealing with toxæmia and sepsis, there is much which invites criticism.

In the opening remarks: "Clinicians and pathologists have adopted the habit of endeavouring to explain certain symptom complexes," etc., would be nearer the truth if put in the following words: "Clinicians and pathologists have, from clinical experience, been forced to assume the presence of an infecting agent, which they would localize in an ascertained septic focus." The other is rather like the Irishman trailing his cloak on the ground.

In this article the word "toxin" is used as acting at some distance from the bacteria. Rupert Farrant showed, a few years ago, that thyroid enlargement could be produced by the injection of many bacterial toxins, also by abrin and ricin. It is also inconceivable to think that in gross facial toxæmia with anæmia that there are no organic changes in the body, such as myocardial degeneration, etc.. I see no reason why a toxin must necessarily produce pyrexia, as small doses constantly given will produce a certain amount of immunity and so give no constitutional symptoms. We do not get pyrexia in the ordinary doses, as used in vaccine treatment. I do not think the aspect of the toxin acting at a distance is seriously entertained by many, except in diphtheria and tetanus, as it is much more reasonable to assume that the bacteria themselves gain entrance to the blood or lymphatic stream and eventually lodge in some tissue, providing they escape destruction on the way. Having come to rest, toxins are liberated, resulting in some degree of inflammation, according to its virulence. Here it is not the mechanical presence of the bacteria which does any harm, unless they are in a mass large enough to block a small vessel, but due to the toxin liberated.

In view of the fact that there is no tissue of the body that

can claim immunity from invasion by infective organisms, the question at stake ought to be: "Can anybody bring evidence to prove that iritis is due to any factor other than an infective organism?" I fully agree that it would be absurd to think that the iris had a selective action for a certain toxin. But in the light of certain work that has been done fairly recently, it is very reasonable to assume that the iris may have a specific elective affinity for certain strains of the streptococcus group. Surely if the cornea is capable of being involved by the spirochæte, the iris is also capable of being involved by a streptococcus or any other infective organism and also that if an individual is breeding streptococci year after year in a cavity at the apex of a tooth, the chances are far more in favour of a blood or lymphatic infection of the iris than by a casual infection any other way. We have also this fact to bear in mind. that the immunity mechanism is liable to deteriorate and so allow of bacteria being carried about in the blood stream without destruction.

Diseases and tissue changes are due either to bacillary invasion, metabolic changes or deficiency factors, but never have they been found due to two of these causes. As in the laws of averages, the iris must get involved by bacteria and such cases must come before ophthalmologists and the resulting inflammation must be obvious and take the form of the iritis to which we are accustomed, as we know of no other.

Now, referring to Dr. Pockley's paper of February 28, he quotes a report from the Wills' Eye Hospital of 500 cases of iritis, giving the causes. If we substitute that expression, "giving the causes," and say "associated with the following diseases," it will be more rational, twelve of which are undoubtedly due to bacterial infection. The heading "exposure" can be put on a par with pneumonia, being due to exposure and consequently classed as unknown causes. Diabetes is also quoted, as are other organs, so is the pancreas liable to infection, and sufficient cases of diabetes have been cured by removal of a septic focus, to suggest strongly that a certain proportion of such cases are due to that cause. Gout is in the melting-pot. Personally, I have not come across a case without a septic focus during the last few years. Lead poisoning is often associated with pyorrhœa, so in practically all of the 500 cases there was an associated bacterial invasion. It is an extremely interesting coincidence that of 500 cases there were so few not definitely associated with some bacterial invasion and it is more than probable no septic focus was looked for in the others.

The one common factor to all these cases is that there was bacterial infection and that practically any organism invading the blood stream is liable to lodge in the iris and set up proliferative changes.

It is quite inconceivable that a disease can be due both to bacterial invasion and also due to imperfect metabolism; it must be due to one or the other.

Re rheumatic diathesis. Does that mean some metabolic process, or does it mean a family susceptibility to or rather a lack of resistance to a certain infection? It must be one or the other and there is much evidence to show that rheumatic conditions are due to bacterial invasion and no evidence to show it is due to any other factor, so the onus now is for someone to produce a case of arthritis which does not show some bacterial infection and can be proved to be due to some other factor.

Dr. Pockley suggests that rheumatic iritis may be of the nature of a myositis, fibrositis of muscle or connective tissue. In that I fully concur, but not in that they can be caused by chills, exposure, errors of diet, with imperfect metabolism of proteins, as myositis and fibrositis are due to direct invasion by bacteria and are readily flared up by chills, exposure, acid wine, etc., the same as mildly infected joints.

Quoting Dr. Pockley again: "It is generally accepted that 95% to 98% of civilized people suffer with pyorrhœa or other dental trouble and iritis so rare." If we take the cubic capacity of the iris and compare it to the cubic capacity of the rest of the body, it will not be difficult to see why iritis is so rare. After the age of 40 it is very rare to find a thoroughly healthy person, as so few have escaped some damage, however small, to some or other organs of the body and then it must be remembered that it is only during the last year or two that pyorrhœa has been recognized as having any harmful influences. But it is probably near the truth that pyorrhœa does more harm in the community than syphilis and tubercle put together.

Referring again to the sub-leader: "In rheumatism of all kinds it is possible that the infecting organism that induces arthritis and the changes in the heart is the same as that which produces tonsillar changes." It is not outside the realms of possibility for the joints to be infected with the tubercle bacillus, the heart with a gonococcus and the tonsil with a streptococcus, but if the heart is infected with a streptococcus, its portal of entry would most frequently be over or *viâ* the tonsils and post-pharyngeal adenoid tissue and the odds are that both would be infected by the same strain of bacteria. "The involvement of the tonsil in acute rheumatism, however, should not be regarded as a cause." It matters not where the organism gains entrance, but gain entrance it must. A focal infection anywhere will do. I have seen a most severe valvulitis from a scratch on the arm. But how much more likely to be the cause is a focal infection which is constantly present, or a chronic one which flares up from time to time than a casual one.

"It has not been proved that a tonsil infected with a pyrogenic organism is capable of giving rise to changes elsewhere in the body other than those of general intoxication and pyrexia."

One may as well say it has not been proved that a tubercular mother infected her children or her husband, who have later gone down with that disease, or that the bacilli which caused tubercular peritonitis in a child already suffering from phthisis, did not come from the lungs but from some outside source; both are possible.

How is this to be proved and what constitutes proof? By this must be meant: Can it be proved that organisms do not gain entrance to the blood or lymphatic stream from the tonsils or if they do gain entrance, they are immediately killed or rendered harmless, because, failing this, they must set up some inflammatory change where they lodge?

First of all: Can they gain entrance to the blood or lymphatic stream? We know that organisms of the streptococcus group, as well as most others, are capable of wandering about the body, being carried by blood or lymphatic stream. From a septic focus such as the tonsils they have two paths of entry, directly into the blood or lymphatics and by swallowing into the stomach. If a child has valvular disease of the heart, organisms had to gain entrance somewhere. What is the most likely and therefore the commonest portal of entry? Through the nose or the mouth. Now, the large majority of bacteria must of necessity lodge on the mucous membrane of the throat and nose. If in the nose, the mucus and nasal secretions will deal with them to the best of their ability or pass them on. The adenoid tissue of the pharynx and tonsils, by means of their phagocytes, will seize upon them in the naso-pharynx. Some will be swallowed. If the invasion is small it may be wiped out; if not, after an acute attack of tonsillitis, in which a certain immunity is gained. it settles down to a chronic condition in which infected material can be squeezed from the crypts.

From this time on over many years this condition remains, valvular and myocardial diseases develope, joint changes take place and with every flare-up of the throat joint pains are accentuated. As time goes by the immunity mechanism, which at first may be able to destroy bacteria as they gained entrance to the blood stream, gradually falls and allow of these organisms to gain ground and multiply, as seen in advanced valvular and myocardial diseases. A gland infected with tubercle bacilli can and does infect the whole body. Pyæmia can arise from a small focus. Is there any reason to suppose that an infected tonsil keeps all the bacteria it incubates to itself; if it destroyed them the tonsil would cease to be septic; if not, they must be swallowed or go directly into the blood or lymphatic stream, and yet the statement is made? "It has not been proved that a tonsil infected with a pyrogenic organism is capable of giving rise to changes elsewhere in the body other than those of general intoxication and pyrexia."

If it cannot be proved, it is not within the realms of common sense to think that virulent or only mildly virulent organisms can be swallowed day after day, month after month and year after year, or gain entrance to the blood stream without giving rise to changes elsewhere, as we know that those actual organisms are found within the body where

changes have already been produced and that they have had to go the route as described above. Any focal infection is equally potent, even an infected prostate gland or onychia. If one gets repeated cures of diseases by the removal of septic foci which have not responded to treatment without, that, together with the recent work done by Billings and others, of reproducing these conditions in animals by injecting micro-organisms taken from septic foci, should be proof enough.

It has been argued that on no occasion has a culture been made from iritis. Iritis, if it were due to bacteria, is the result of the presence of bacteria and not the presence. The bacteria may die but the result persist for a time, at least. According to the strength of the immunity mechanism, so will be the power of a bacterium to remain alive within the blood or tissues. If bacteria by chance lodge in the iris and the immunity mechanism was so low as to allow of their multiplying indefinitely, pyæmia would result; if it were only fairly strong the bacteria would persist at least for a little while and even multiply to a certain degree.

Since the subject of focal infection has been in vogue, it has been noticed that in many cases the removal of a septic focus has often cured the disease. This suggested, and reasonably, too, that supplies of fresh bacteria were necessary to keep up the condition, as without them the disease dies out, showing that the tissues were able to deal with those brought to them, but only after some damage had been done. On such grounds one would not expect to be able to isolate live bacteria in this class of infection.

Yours, etc.,

SYDNEY PERN.

[Dr. Pern occupies a great deal of space in expounding theories without adducing any evidence. He states that in the leading article referred to, the word toxin was used to signify a substance acting at some distance from the bacteria. A toxin to produce changes, must be soluble. If it is capable of solution, it would necessarily pass into the blood or lymph channels and could then exert its action on any tissue of the body. Dr. Pern has misunderstood the statement made concerning the production of pyrexia. A bacterial toxin is not identical with the disintegration products of bacteria. There is a vast amount of experimental evidence to show that pyrexia is always due to the chemical stimulation of the heat-regulating apparatus. The immunization by vaccines inhibits the disintegration of the protein into those amino-acids as end-products which act as chemical stimuli. In the next place, Dr Pern introduces an irrelevant issue by referring to organisms other than those capable of producing suppuration, for the whole question is centred around the contention that a septic focus is the cause of a large variety of affections, such as iritis, exophthalmic goitre, acute rheumatism, etc.. Dr. Antill Pockley is at present travelling to England and cannot reply to the criticism. We feel justified in stating that he has established beyond dispute that the association of common affections, such as a carious tooth or an infected tonsil and rheumatism, cannot be accepted as evidence of cause and effect. We hold that it is probable that an acute rheumatism is due to a bacterium. This bacterium may gain entrance into the body through the tonsil. But there is nothing to suggest that all or even the majority of tonsillar infections are caused by the hypothetical bacterium of rheumatism, nor that this hypothetical organism produces any changes in the tonsil. Dr. Pern fails to recognize that a tubercular infection can be demonstrated and in many instances its source traced. When a tonsil is infected by a pyrogenic organism, it is safe to assume that, as long as the bacteria do not break through the barrier and give rise to a general septicæmia, the only result will be a poisoning due to the split products of the bacterial proteins. In spite of much searching, no one has been able to demonstrate a specific endotoxin of a streptococcus. Moreover, every experienced bacteriologist has learned that bacteria have difficulty in retaining their lives when in the blood stream. If a small quantity of a virulent coccus be injected into the vein of a rabbit, it is rare indeed to be able to recover the organism from the blood ten minutes later. In the last place, Dr. Pern's contention that "the disease" can be cured by the removal of a septic focus is unconvincing. The statement is too general. It still remains to be proved which bacteria, if any, cause an iritis, which cause an exophthalmic goitre, which cause an acute rheumatism. If the source of infection can be traced and removed or destroyed, naturally the disease would be arrested. But we do not know any of these things and our knowledge is not advanced by postulating a hypothetical toxin or an assumed bacterium.—ED.]

THE HOME OF THE NEW SOUTH WALES BRANCH.

Sir: Some years ago when the Council of the New South Wales Branch of the British Medical Association determined that we should have a home of our own in which to hold meetings, to store a library and provide offices in which the work of the Branch could be comfortably carried on, there were some pessimists who thought we could not afford the expense and that it was not a good business proposition. Many men who could well afford to do so, did not take debentures. Unfortunately, this cramped our enterprise. If we had then obtained sufficient funds, a very suitable site in Macquarie Street could have been acquired. What would now be our unearned increment? Probably three-fold! Our Elizabeth Street property has also increased in value. As a business site it is probably more valuable than Macquarie Street; for our purposes it is less so. It is too noisy. We can probably sell it at a good profit. Let us do so. The suburban practitioner who, when our Elizabeth Street home was built, lived and practised only in his suburb, has now come to Macquarie Street in force as a part-day migrant. It is to his interest also to have a rest for the sole of his foot. Now is our opportunity. Let us combine to house ourselves in an up-to-date, modern building, where the Branch will be more conveniently placed and where, while a good return on the investment is sure, our members will not be at the mercy of rack-renting landlords. They will have comfortable, sanitary rooms in which to work. We might consider joining with the dental profession in this enterprise. They are equally interested.

I pray that this vital matter may not be allowed to drift into mere talk and letter writing. Let us be men of action; otherwise the younger generation will have cause to regret.

Yours, etc.,

OLD HAND.

Sydney, April 2, 1920.

THE LUNACY SERVICE IN SOUTH AUSTRALIA.

In our issue of March 27, 1920, some information was given in regard to the conditions of service in the hospials for the insane in the several States. In the table published in connexion with this article, the entries for South Australia were left blank, except in regard to the office of Medical Superintendent.

There is one mental hospital in South Australia at Parkside. This hospital is under the control of the Inspector-General of the Insane, who has also the direction of the institution for mental defective children. At the Parkside Hospital there are at present the following medical officers:—

Superintendent; salary £750 with house, fuel, light and rations.

Deputy Superintendent; salary £575, with house, fuel, light and rations.

Junior Medical Officer; salary £500, with quarters, fuel, light and rations.

Pathologist; the Professor of Pathology of the Adelaide University acts as Pathologist to the Mental Hospital.

THE AUSTRALASIAN MEDICAL CONGRESS, BRISBANE, AUGUST, 1920.

The Executive Committee of the Australasian Medical Congress appeals to members who intend to be present at the Congress (August 23-28, 1920), to communicate as soon as possible with the Honorary Secretary in the State in which they reside. The Secretaries are prepared to secure accommodation for members and their wives either in hotels or in boarding houses. Those who apply late, may experience

difficulty in regard to accommodation. The Executive Committee would wish to be informed when members are making their own arrangements.

Medical Appointments.

Dr. Winifred L. Dillon has been appointed Medical Officer in the Medical Branch of the Department of Education of New South Wales.

The resignation by Dr. J B. Cleland (B.M.A.) of his position as Principal Microbiologist in the Department of Public Health of New South Wales is announced in the *New South Wales Government Gazette*. Dr. Cleland has been appointed Professor of Pathology in the Medical School of the University of Adelaide.

Dr. E. A. Woodward (B.M.A.) has been appointed Government Medical Officer at Emmaville, New South Wales.

The Council of the Shire of Dunmunkle, Victoria, has approved of the appointment of Dr. F. T. Wheatland (B.M.A.) as Officer of Health for the East and West Ridings of this Shire.

The appointment of Dr. J. J. Black (B.M.A.) as Public Vaccinator for the Metropolitan District and of Dr. Cyril Checchi (B.M.A.) for the South-Western District of Victoria, is announced in the *Victoria Government Gazette*.

Dr. E. W. Ferguson (B.M.A.), Acting Principal Microbiologist, has been appointed a member of the Advisory Committee for the purposes of the *Pure Food Act, 1908*, of New South Wales, in place of Dr. Cleland (B.M.A.) who has resigned.

For a term of three years Dr. J. B. Moore (B.M.A.) and Edmund T. Webb, Esq., J.P., have been appointed members of the Licensing Court for the Licensing District of Bathurst, New South Wales. The appointment dates from March 31, 1920.

Medical Appointments Vacant, etc.

For announcements of medical appointments vacant, assistants, locum tenentes sought, etc., see "Advertiser," page xxi.

Melbourne Hospital: Various Vacancies on the Honorary Medical Staff.

Royal Alexandra Hospital for Children, Camperdown, Sydney: Various Vacancies on the Honorary Medical Staff.

Medical Appointments.

IMPORTANT NOTICE.

Medical practitioners are requested not to apply for any appointment referred to in the following table, without having first communicated with the Honorary Secretary of the Branch named in the first column, or with the Medical Secretary of the British Medical Association, 429 Strand, London, W.C.

Branch.	APPOINTMENTS.
VICTORIA. (Hon. Sec., Medical Society Hall, East Melbourne.)	All Friendly Society Lodges (other than the Grand United Order of Oddfellows and the Melbourne Tramways Mutual Benefit Society), Institutes, Medical Dispensaries and other Contract Practice. Australian Prudential Association Proprietary, Limited. Mutual National Provident Club. National Provident Association.
QUEENSLAND. (Hon. Sec., B.M.A. Building, Adelaide Street, Brisbane.)	Australian Natives' Association. Brisbane United Friendly Society Institute. Cloncurry Hospital.
SOUTH AUSTRALIA. (Hon. Sec., 3 North Terrace, Adelaide.)	Contract Practice Appointments at Renmark. Contract Practice Appointments in South Australia.
WESTERN AUSTRALIA. (Hon. Sec., 6 Bank of New South Wales Chambers, St. George's Terrace, Perth.)	All Contract Practice Appointments in Western Australia.
NEW SOUTH WALES. (Hon. Sec., 30-34 Elizabeth Street, Sydney.)	Australian Natives' Association. Balmain United Friendly Societies' Dispensary. Friendly Society Lodges at Casino. Leichhardt and Petersham Dispensary. Manchester Unity Oddfellows' Medical Institute, Elizabeth Street, Sydney. Marrickville United Friendly Societies' Dispensary. Newcastle Collieries—Killingworth, Seaham Nos. 1 and 2, West Wallsend. North Sydney United Friendly Societies. People's Prudential Benefit Society. Phœnix Mutual Provident Society.
NEW ZEALAND: WELLINGTON DIVISION. (Hon. Sec., Wellington.)	Friendly Society Lodges, Wellington, New Zealand.

Diary for the Month.

Apr. 13.—Tas. Branch, B.M.A..
Apr. 13.—N.S.W. Branch, B.M.A., Ethics Committee.
Apr. 14.—Vic. Branch, B.M.A..
Apr. 14.—North-East. Med. Assoc. (N.S.W.).
Apr. 15.—Vic. Branch, B.M.A., Council.
Apr. 16.—Eastern Suburbs Med. Assoc. (N.S.W.).
Apr. 17.—Northern Suburbs Med. Assoc. (Sydney).
Apr. 20.—N.S.W. Branch, B.M.A., Executive and Finance Committee.
Apr. 21.—W. Aust. Branch, B.M.A..
Apr. 22.—Q. Branch, B.M.A., Council.
Apr. 27.—N.S.W. Branch, B.M.A., Medical Politics Committee; Organization and Science Committee.
Apr. 28.—Vic. Branch, B.M.A., Council.
Apr. 29.—S. Aust. Branch, B.M.A..
Apr. 29.—Western Suburbs Med. Assoc. (N.S.W.), Annual.
Apr. 30.—N.S.W. Branch, B.M.A..
May 5.—Vic Branch, B.M.A..
May 7.—Q. Branch, B.M.A..

EDITORIAL NOTICES.

Manuscripts forwarded to the office of this journal cannot under any circumstances be returned.

Original articles forwarded for publication are understood to be offered to *The Medical Journal of Australia* alone, unless the contrary be stated.

All communications should be addressed to "The Editor," *The Medical Journal of Australia*, B.M.A. Building, 30-34 Elizabeth Street, Sydney. (Telephone: City 2645.)